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ALCOHOL AND SKILLED PERFORMANCE

By G. C. DREW, M.A.
Professor of Psychology in the University of London

Weekly Evening Meeting, Wednesday 1st June, 1960

L. B. W. Jolley, M.A., M.I.E.E.
Vice-President, in the Chair

THERE seems to be little disagreement that at blood alcohol levels of 0.1% and above almost all aspects of behaviour which have been studied show clear evidence of deterioration. There is, however, no comparable agreement on the effects of low blood alcohol levels, especially of those of the order of 0.05% and below. In part, of course, this is to be expected, since the effects of behaviour of low concentrations of any drug are likely to be less clear-cut and less definite than those of higher concentrations. The problem of determining the kind of effect such blood alcohol levels have is a more complex one at these lower levels, and demands greater attention to method, and greater care in research design than has, with certain notable exceptions, been common in this research field.

This is a pity, since it is important both from a theoretical and a practical point of view, to have more definite knowledge than we have at present. In England, it is a widely accepted belief that driving is actually improved, for some people at least, by alcohol in this sort of concentration. The work of Cohen, Dearnally and Hansel is perhaps relevant here. Two white posts, erected some distance in front of experienced bus drivers seated in their buses, were moved together or apart. Drivers were asked first to say what was the narrowest gap they could drive the buses through, and secondly, to do it. After 6 fluid gs. of whiskey, some were quite confident of their ability to drive an 8 ft. bus through a gap less than 7 ft. wide, though, in fact, it needed to be nearly 9 ft. wide before they could do so. This lowering of subjective standards of critical judgment is, I believe, an important effect of low blood alcohol concentrations.

In reviewing the literature, one finds evidence supporting any of three hypotheses on the effects of low blood alcohol concentrations on behaviour. The first is that behaviour in a skilled task

begins to deteriorate as soon as there is any measurable blood alcohol. The second is that there exists what Professor Goldberg called a "critical level," a threshold below which alcohol has no effect and above which it produces deterioration. The third is that skill improves at low concentrations, only deteriorating at levels above, say 0.05%.

The experiment I am reporting here was an attempt to obtain further evidence on these hypotheses. In designing it, we thought it important to measure blood alcohols at fairly frequent intervals, so that we could relate behaviour changes to the actual level of alcohol present at the time.

To find if there is a threshold we had to test each subject several times with different amounts of alcohol and with a placebo, comparing each man's performance *with* alcohol, with his own *without* it. This brings with it the danger of confusing alcohol and practice effects, so a latin square design, in which each man has a different order of doses, was used.

The last design point to which I want to draw attention now, is that the task used was a long one, lasting over two hours on each occasion. This I believe to be important, since there is a great deal of evidence to show that quite misleading results can be obtained from tests which last only a short time. As long as we know we are being tested, and as long as we think we might be getting less efficient, we can compensate for a short time and may in fact do much better, though we may be unable to keep this up. When measured by short, interpolated tests, fatigue, a noisy working environment, extreme heat, and lack of sleep all tend either to show no effect on behaviour, or show an actual improvement.

The task finally used, therefore, was a driving simulator, difficult enough and interesting enough to keep people working at it for two hours and to bring them back again on subsequent occasions.

All subjects were drivers—35 men and 5 women. All were tested in the mornings, two hours after a fat free breakfast. After preliminary practice, taking a battery of personality tests and so on, each subject was given his appropriate drink, (absolute alcohol, diluted 4:1 and flavoured to hide the alcohol) and then drove the machine for two hours. Blood samples were taken be-

fore and every half hour after. His instructions were to drive on the left hand side of the road, to drive at whatever he thought a reasonable speed and generally to behave as he would on a road.

RESULTS

The change in the mean blood alcohol values for each dose through time is shown in Fig. 1. Following each dose, blood

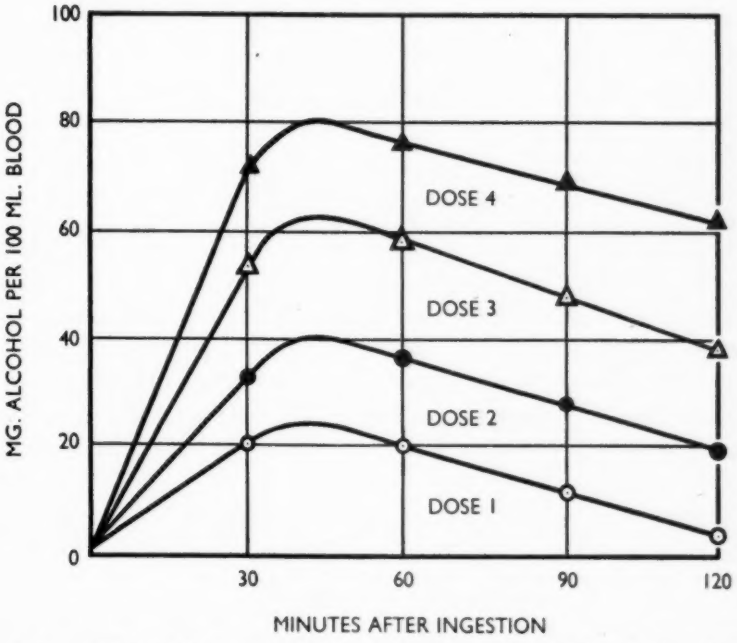


FIG. 1. Relations between the level of alcohol in the blood and time after ingestion, for four levels of alcohol.

alcohol rises steeply to a maximum, reached somewhere between 30 and 60 minutes after drinking, and then falls off in an approximately linear way. There is a tendency for the peak concentration to occur slightly later as the dose increases. That following the largest dose gives a mean time of nine minutes later than that for the smallest. For some subjects, measurements were taken up to six hours after drinking. Dose 1 fell to near zero concentration by the end of two hours, dose 2 at about four hours, dose 3 at rather over five hours, and at six hours the mean concen-

tration following dose 4 was still 20 mg. per 100 ml. Once the peak was passed blood alcohol levels fell, on average, by approximately 15 mg. per 100 ml. per hour.

PERFORMANCE CHANGES

The aspects of performance measured concerned the accuracy of tracking, the speed at which the task was taken, and the operation of the controls.

The accuracy measures consisted, firstly, of tracking error (the amount of deviation across the road surface from a track parallel to the left-hand kerb) and kerb-bumpings (the more serious error of colliding with the side of the road). Secondly, two detailed accuracy measures were obtained—positioning of the car relative to the left-hand side (this was measured on a sample of 10 subjects for no-alcohol and dose 4.), and consistency in car positioning at a corner in the circuit (measured, for 35 subjects, as the range of differences in negotiating the same corner on six successive occasions in the second driving period under each dose condition).

Speed was recorded as the number of seconds taken to complete one lap. The total amount of steering-wheel movement made will be the only aspect of control movements considered here.

All scores were expressed as a summed score per lap, and were then averaged over five minutes. The five-minute averages were then summed over twenty minutes. They were subjected to a variety of statistical treatments, including analysis of variance. Regression lines and coefficients of correlation are used here.

The results are presented in terms of group effects, followed by some discussion of individual differences in response to alcohol and their relationship to other personal characteristics.

A. GROUP EFFECTS

Practice effects varied in the different performance measures. Tracking error showed very little change with practice, but there was a progressive increase in speed of driving throughout the experiment. Time per lap and tracking error were negatively correlated ($r = -0.39$), but correction of error scores for increased speed makes little difference to the overall picture. Steering-

wheel movement shows a pronounced practice effect in a progressive and marked reduction in the amount of movement throughout the experiment.

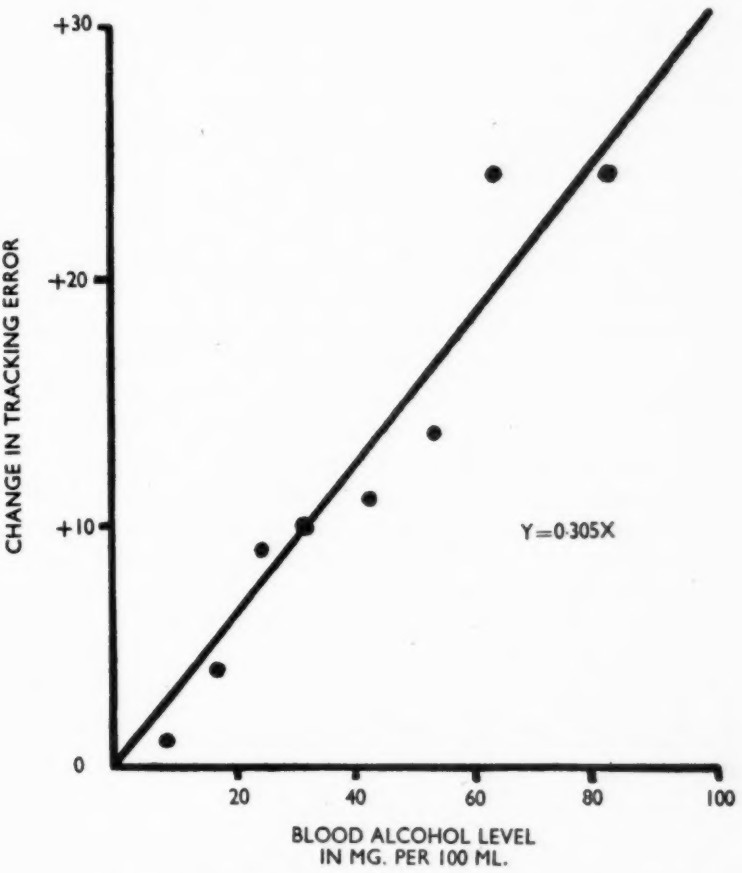


FIG. 2. Regression of tracking error on blood alcohol level.

Effect of Alcohol on Accuracy

Accuracy of steering, in a complex task of this kind, decreases progressively as blood alcohol increases, even with the low concentrations used in this experiment. Furthermore, there is, from the group effects, no evidence of a threshold. Instead, there is a

measurable increase in mean error as soon as there is a measurable quantity of alcohol in the blood. The relation between accuracy, expressed as tracking error, and blood alcohol is shown in Fig. 2. This shows the regression of error on blood alcohol. The regression is highly significant ($p < 0.001$). This is confirmed by an analysis of variance, which shows a relation of error and blood alcohol significant at better than the 0.01 level of probability. The mean increase in error for the 80 mg. per 100 ml. concentration compared with no-alcohol performance is about 16%.

It has been noted above that blood alcohol concentration shows a characteristic change with time after drinking showing, first, a steep rise to a maximum value, followed by a slow return to normal. Tracking error shows a very similar pattern with time after drinking. The close correspondence of the rise and fall through time of blood alcohol and tracking error is shown in Fig. 3. In this Figure, tracking error scores are expressed as the mean score for each driving period. Blood alcohol concentrations have, therefore, been expressed in the same way. The rise and fall of urine alcohol, expressed similarly, have been included. The figure for urine alcohol has been reduced by one-fifth (in view of the ratio of 1.252:1) to enable it to be presented on the same scale. Although only the largest dose has been plotted, the data for the other doses follow the same pattern.

It can be seen that urine alcohol reaches a maximum about 20 minutes later than blood alcohol. The error curve looks as though its peak would occur somewhere between the two. The calculation of lag correlations confirms this. Maximum correlations are obtained when error scores are correlated with blood values some 10 minutes earlier, and with urine values some 10 minutes later. From this it would appear that maximum error slightly lags in time behind the maximum blood alcohol and slightly precedes maximum urine alcohol.

Product-moment correlation of the rise and fall of blood alcohol and the rise and fall or error is positive and significant. The mean correlation of the four paired values, calculated separately for each individual, is $r = +0.271$, with p lying between 0.01 and 0.02. The size of this correlation, though significant, seems small. It will be seen from Figure 3 that there is a time lag between the two variables with an inverted relationship in

periods 2 - 3. This, and the clustering of points in the second, third and fourth driving periods, probably have reduced the size of the correlation. When blood alcohol values at five-minute intervals, obtained by interpolation from the individual curves, are correlated with the five-minute error scores, allowing a much smaller lag than the twenty-minute scores, the correlation rises

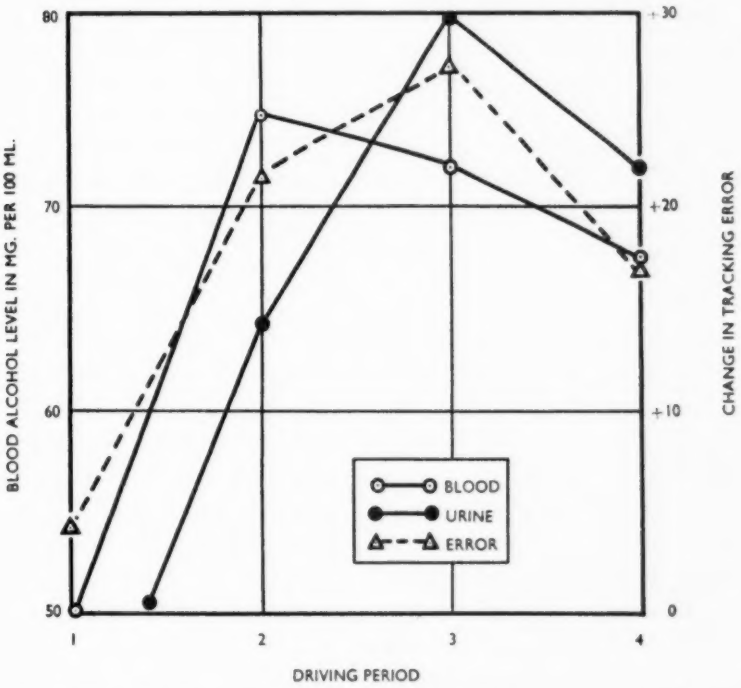


FIG. 3. Rise and fall of blood alcohol, error score, and urine alcohol with time after drinking. (Urine alcohol levels reduced by one-fifth.)

to r = + 0.89. Because the test ends before blood alcohol has dropped very markedly, it is not possible from these scores to say whether error scores recover more quickly than blood alcohol levels, though inspection of Fig. 3 shows that the slope of error scores in the last half hour is steeper than for blood alcohol. The frequency of kerb-bumping rises steeply to a maximum for each dose with blood alcohol, and falls equally steeply after the maximum has been passed.

In an attempt to interpret the increased error with increased blood alcohol, records were sampled for mean position on the road surface, amount of swing about this position, and consistency of behaviour on corners. Significant differences were found. Without alcohol, subjects tended to drive on the left-hand side of the road, to remain on that side for relatively long periods—that is, with relatively little "wobble"—and rapidly to correct swings to the right by moving back to the left once more. The effect of alcohol appears to be to produce a shift of the mean driving line from the left towards the crown of the road ($p<0.01$). With increasing dosage there is an increasing swing or "wobble" about this new, more central position ($p<0.01$), together with a tendency to tolerate swings to the right but rapidly to correct swings to the left ($p<0.01$). There was, furthermore, greater variability in the positioning of the car in negotiating a corner ($p<0.01$).

Effect of Alcohol on Speed

In this experiment there were no clear-cut group effects on speed of driving. Mean driving speeds are insignificantly different for all doses. Variability in speed around a mean shows a tendency to rise and fall through time with blood alcohol rise and fall, but this again is not significant. It is possible that the large effect of practice on speed is camouflaging any alcohol effect for the group. Changes in speed of driving after alcohol were found to be closely related to personality characteristics. These are discussed below.

Effect of Alcohol on Steering-wheel Movement

Like error, steering-wheel movement increases progressively as blood alcohol increases. The regression of amount of change of steering-wheel movement on blood alcohol is linear and is highly significant ($p<0.001$). There is also a decrease in the consistency of steering-wheel movement after alcohol ($p<0.01$). The decrease in consistency appears to be due to a greater variability in the timing of responses to a corner.

As might be expected, the amount of steering-wheel movement is related to the amount of tracking error made. The relationship is U-shaped, such that there is an optimum amount of movement

TABLE I

Number of Subjects Showing Different Percentage Change in Scores after Alcohol (Dose 4)

Measure	*Percentage Change of Score*											
	Decrease					*Increase*						
	−50	−40	−30	−20	−10	+10	+20	+30	+40	+50	+60	+70
Error	1	0	2	2	5	10	12	4	2	2	—	—
Steering-wheel movement	—	—	—	3	5	12	13	3	3	—	—	1
Time	—	—	—	5	11	13	5	2	—	—	—	—

for controlling the car, both less and more movement resulting in increased error. Following alcohol there is an increasing tendency to make either too little or too much movement in negotiating a particular corner, though the predominant effect is to make too much.

B. INDIVIDUAL DIFFERENCES IN RESPONSE TO ALCOHOL

Although the latin square design used makes comparison of the performance of individuals difficult, certain features nevertheless emerge. There is undoubtedly a considerable range of individual variation in response to alcohol. This is summarized in Table I, which gives the number of subjects changing their score after alcohol by varying amounts. The changes are expressed as percentage change from their no-alcohol score. Tracking error shows a wide scatter. Though the distribution is fairly heavily skewed towards increased error after alcohol, 10 of the 40 subjects show reduced error, one as much as 50% reduction. This subject, however, showed also one of the largest reductions in speed, and achieved a high degree of accuracy by driving extremely slowly. Time scores, on the other hand, are distributed normally around the "no change" position.

An attempt has been made to relate these individual differences in response to alcohol to various personal characteristics. No relationship was found between response to alcohol and differences in initial level of skill, previous driving experience, age, sex or drinking habits. The subjects varied widely in their initial level of skill and in previous driving experience, so the lack of relationship between these variables and alcohol effect may be stated with some confidence. The range of drinking habits, however, was small, so that the comparison was between the few individuals who drank once or twice a week and those who drank very rarely. The range was, therefore, too small to draw conclusions on the lack of relationship.

A relationship was found, however, between individual differences in the effect of alcohol and scores on tests of extraversion-introversion. Performance scores were compared for a group of subjects scoring high in extraversion and a group scoring low. Figure 4 shows the mean differences in tracking performance

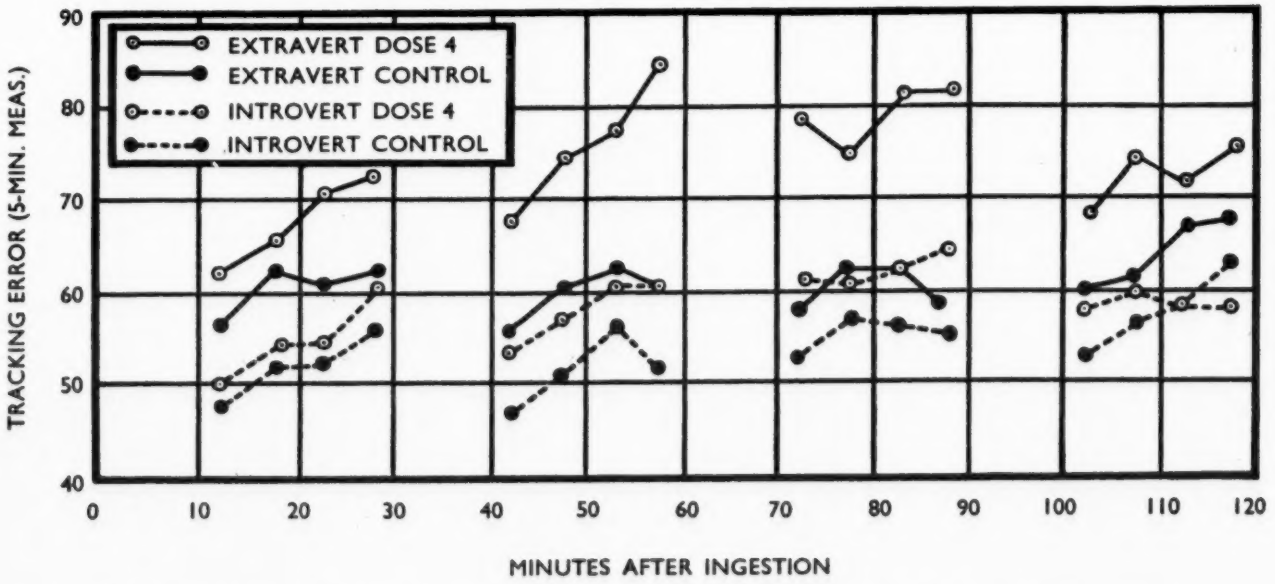


FIG. 4. Comparison of the tracking error of extraverts and introverts under no alcohol and dose 4 conditions.

between the resulting seven extraverts and nine introverts. The extravert group made more error ($p < 0.05$), were less consistent in car positioning ($p < 0.05$), and showed a bigger increase in error during each period of driving ($p < 0.01$). The effect of alcohol on error was greater than for the introvert group ($p < 0.05$), the extraverts having an average increase of 23% and the introverts of 6%.

The extraverts responded to alcohol in a similar manner, but the introverts showed more varied responses. For example, the extraverts all increased their tracking error after alcohol, whereas two of the introverts made less error. This is especially clearly shown in the effect of alcohol on speed of driving. The extraverts showed very little change of speed after alcohol, but the introverts changed considerably. The nearer the introvert end of the scale they were, the more the speed changed. They subdivided, however, into two distinct groups, one of which speeded up very considerably, and the other slowed down. It has not so far been possible to relate this to any personal characteristics. Figure 5 shows the regression of change of speed on extraversion-introversion as measured by the Bernreuter Neurotic Inventory. This regression is linear and is significant at the 5% level. Comparing those scoring more than 50 on the Bernreuter with those scoring less than this in terms of whether they change speed by more or less than 8 units gives a X^2 significant at better than 0.01. It would appear that personality characteristics —at least those of the extravert-introvert dimension—are related to the effects of alcohol.

DISCUSSION

It is now generally agreed that a relationship exists between the concentration of alcohol in the blood and the appearance of clinical signs of intoxication. The definition of intoxication based on clinical evidence is, however, a variable one. Liljestrand (1940) gives evidence of a considerable variation in diagnoses made by different clinicians on people who all had blood alcohol concentrations between 100 and 150 mg. per 100 ml. Moreover, the range of blood alcohol concentrations at which people are judged intoxicated is very wide. Goldberg (1951) has shown that the level at which 50% of people are judged clinically to be intoxicated varies with the legal definition in different countries.

The level of alcohol in the blood at which people are diagnosed clinically to be intoxicated, besides being variable, tends to be high in comparison with the concentrations used here. This is not surprising, since an estimate of impaired behaviour is made in the absence of any criterion of normal behaviour for that individual, so that the impairment must be obvious before it is

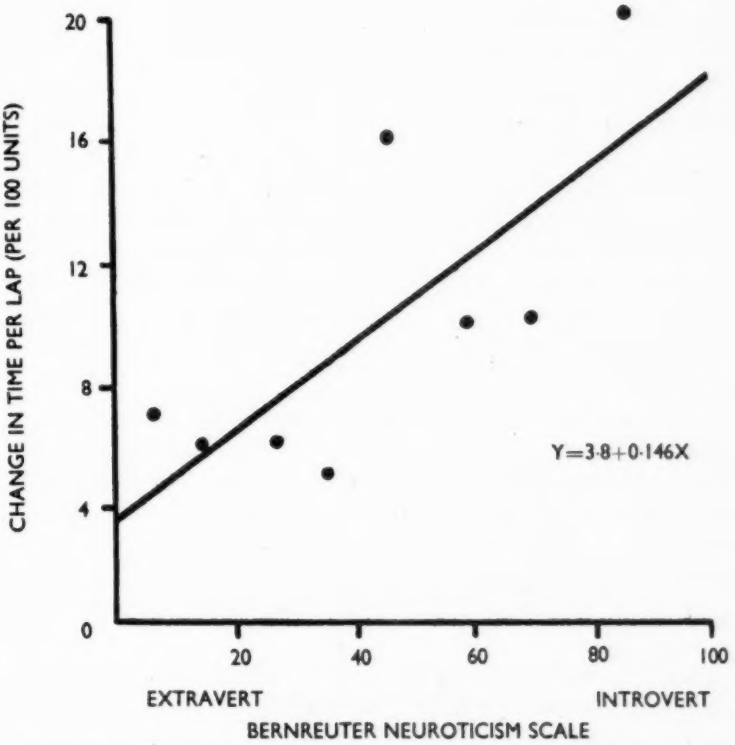


FIG. 5. Regression of change of speed after alcohol on extravert-introvert score.

detected, and since it is known that people can compensate for their reduced efficiency over the short periods of time during which they are examined. It does not follow, however, that because no impairment is found in clinical tests, more complex skills, like driving, will also be unaffected.

The present study shows that performance begins to deteriorate with very low blood alcohol concentrations, certainly of the

order of 20—30 mg. per 100 ml. and that the deterioration is progressive and linearly related to blood alcohol level. There is, in this study, no evidence of a threshold effect.

The impairment of performance was shown most clearly in the operation of the controls. As the aim of the task was primarily to track, it is perhaps not surprising that in attempting to retain a level of accuracy the operation of the steering-wheel should be most affected. Efficiency in using the steering-wheel is indicated by the amount of tracking error which results. With practice the amount of steering-wheel movement made goes down without a reduction in the accuracy of tracking, whereas after alcohol tracking error increases despite an increase in steering-wheel movement. This suggests that timing of the steering-wheel movements was upset. The decrease in consistency of the movements required to negotiate a corner supports this view. The importance of timing of control movements in a complex skill has been stressed previously by Bartlett (1943), and adverse conditions such as fatigue has been shown to have a marked effect on timing.

The work of Cohen *et al.* (1958) is interesting additional evidence. While the task used in this present study reasonably reflects the tracking aspect of driving, it is almost completely free of hazard and risk-taking. These authors have shown that an amount of alcohol not much larger than the biggest dose used in this experiment produced a significant increase in the hazards in which drivers became involved. From the two studies it is reasonable to assume that not only will drivers become involved in greater hazards with this amount of alcohol but they will be less efficient in dealing with them. It is suggested, furthermore, that the results reported here show that the level of alcohol in the blood is a good indicator of the extent of impairment of performance.

In view of the known difficulty of clinical estimates of impairment in such situations, and of the individual's capacity, if he is capable of realizing that he is under test, to compensate temporarily for loss of efficiency, estimation of blood alcohol level would seem the most direct way of assessing impairment. Estimation of the alcohol excreted in urine and in breath allows a close approximation to blood alcohol. In these experiments, urine alcohol

agreed very closely with blood alcohol. Breath analysis, which has enormous administrative advantages over both blood and urine analysis, can, from these results, given a suitable instrument, also give a very close approximation.

It would seem fairly clear, from these results, that the efficiency with which a task like driving is performed is likely to decrease progressively as blood alcohol rises. At some point the loss of efficiency is likely to be large enough to constitute a danger in a practical driving situation. As this experiment has been carried out in a laboratory, necessarily free of the hazards and many of the motivating features of a real-life situation, it is not possible, from these results, to say at what level of blood alcohol the increased risk of accident would become unacceptable. Some evidence on this is available from the experience of the U.S.A., Denmark, Norway and Sweden, where rather different cut-off levels are used.

Individual differences in response to alcohol appear to be related, at least in part, to personality characteristics, especially those of extraversion-introversion. Eysenck (1953) has collected the evidence for the existence of this dimension of personality and has postulated (Eysenck, 1957a) that extraverts would be more susceptible to the effects of depressant drugs like alcohol. The extraverts in this experiment behaved as a group, and tend to confirm Eysenck's hypothesis. The introverts do not behave as a group. It is of interest that all the scales used agreed on the extravert end but did not agree on the introverts. It is possible that the almost bimodal effect noted on change of speed for introverts reflects the intrusion of some other personality characteristic not tested in this experiment. The test which showed this bimodal effect most clearly was the Bernreuter, which uses a more "neurotic" criterion of introversion than the Maudsley.

In this experiment extraverts appear not to be bothered by the extra stress imposed by alcohol. They drive at much the same speed as before, make very little additional corrective movements, but make much greater error, are less consistent, and deteriorate more rapidly during 20-minute driving periods. Introverts, on the other hand, appear to be striving to compensate for the alcohol effect, and to be anxious to demonstrate their efficiency.

They over-react to the situation, make more corrective movements of the steering-wheel, and change their speed markedly. Some slow right down, presumably in an attempt to achieve accuracy, though they do not necessarily do so, while others appear to be attempting to demonstrate how quickly they can drive, again not always with a proportionate loss of efficiency.

EXHIBITS IN THE LIBRARY

(*a*) Demonstrations of a breathalyser and drunkometer, and diagrams illustrating accident hazards associated with alcohol, lent by *Professor G. C. Drew.*

(*b*) Charts and diagrams illustrating accident rates, lent by *The Director, Road Research Laboratory, Harmondsworth.*

EXPLORING THE OCEAN FLOOR

By M. N. HILL, Ph.D.
Assistant Director of Research, Department of Geodesy and Geophysics, University of Cambridge

Weekly Evening Meeting, Friday 28th October, 1960

H. D. Anthony, M.A., B.Sc., Ph.D.
Vice-President, in the Chair

Introduction

There has been vigorous activity and great success during the past one hundred years in the exploration of the geography and geology of the surface of the land masses of the Earth. More recently geophysics has, through its indirect methods, provided much information about the deep structure. At sea, however, there has until recently been less progress in all aspects of exploration in spite of the area of the ocean floor being about twice that of the area of land. This lack of progress in the past arose from three major causes. First, there is the inaccessibility of the ocean floor and the difficulties of working from a constantly moving sea surface. Secondly, there is the lack of commercial interest in the materials of the ocean floor and, lastly, there is the great expense of ships, men and apparatus for deep sea expeditions.

Nowadays we are more fortunate than our predecessors in that money is more freely available for research which is not immediately and directly concerned with improvement of man's health or way of life. Further, with modern techniques, remote or direct exploration of the geology of the surface of the Earth beneath the oceans is more readily possible.

Instrumentation

As an example of the contrast which exists between the present day and the time, 90 odd years ago, when the most famous *H.M.S. Challenger* set off on her world encircling voyage of exploration, is the measurement of the depths of the sea. Then, it was necessary to lower a lead to the floor of the ocean at each point where the depth was required; this, in 15000 ft (a depth which approximates to that of a high proportion of the ocean floor) took many minutes or even an hour or more. Nowadays,

however, we send off a short pulse of sound once a second and some seconds later the bottom echo from each pulse returns to the ship. By timing these echoes, or recording them on paper with a moving pen and through knowing the speed of sound in water, the depth can be determined with precision. Indeed, on some of the flat oceanic plains, variations in depth of less than 6 ft. in a total depth of 18000 ft. are easily measurable.

Although continuous depth profiles of the ocean floor can now be produced with the echo-sounders and contoured charts made (albeit still inadequate through lack of information), it is still necessary to lower devices from the ship to the sea bed. For obtaining samples of muds or rocks of the ocean floor, it is obviously essential for the ships to be equipped with winches and stout wire ropes. Further, since light only penetrates a few hundred feet into seawater, cameras must be lowered near to the bottom if the appearance of the sea floor is to be obtained. This requirement results in oceanographic research ships having to be specially equipped and at present in Britain we have only one ship, the famous *Royal Research Ship Discovery II*, fitted with a winch which can lower the heavier devices to great depths. She belongs to the National Institute of Oceanography (Plate I).

For rock sampling we still use dredges similar to those made for *H.M.S. Challenger;* the method is crude, sometimes misleading, since there is no certainty that the boulders obtained have not been transported from a great distance, and certainly frustrating in that the dredge frequently returns empty or broken. Nonetheless many successful dredge samples have been obtained, indicating such things as large basaltic volcanoes on mid ocean ridges or outcropping from the deep flat plains, and limestones in the tops of seamounts.

For sampling the soft muds or oozes forming vast areas of the ocean floor, we have corers which can penetrate many tens of feet and which produce long continuous sequences of the soft sediments consisting largely of clays or the minute shells of foraminifera or diatoms which have sunk from the surface waters where the organisms lived.

These cores are of great value in finding out about the past climate at the sea surface; the material collects so slowly that a core 30 ft. in length might have taken 1½ million years to deposit.

A photograph of *R.R.S. Discovery II* entering Leixoes, at the mouth of the Douro.

PLATE I

Any changes in climate, such as occurred during the ice ages, will be reflected by changes in the animal and vegetable remains down the length of a core.

Apart from using devices which must be lowered to the sea floor, there are other methods of exploration by remote observations from the sea surface. For example, there is the echo sounding method of finding the sea depth described above. Likewise, sounds from explosions near the sea surface do not simply provide bottom reflections (as with the less intense sounds of the echo-sounder) but penetrate into the sea floor. Through their tracks being bent by changes in physical properties of the layers

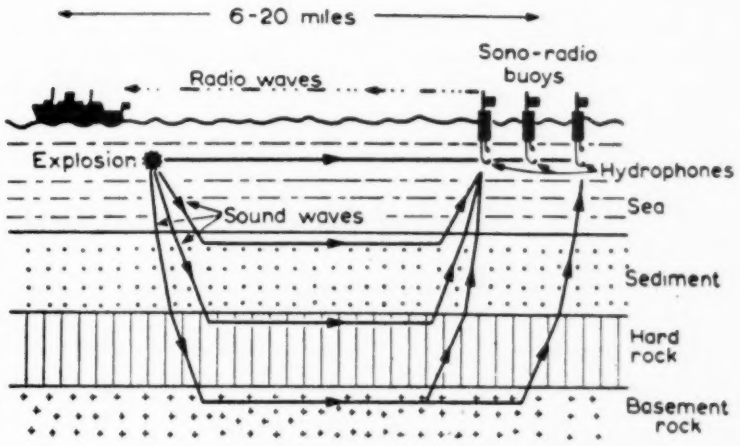


FIG. 1. The sono-radio buoy method of seismic refraction shooting.

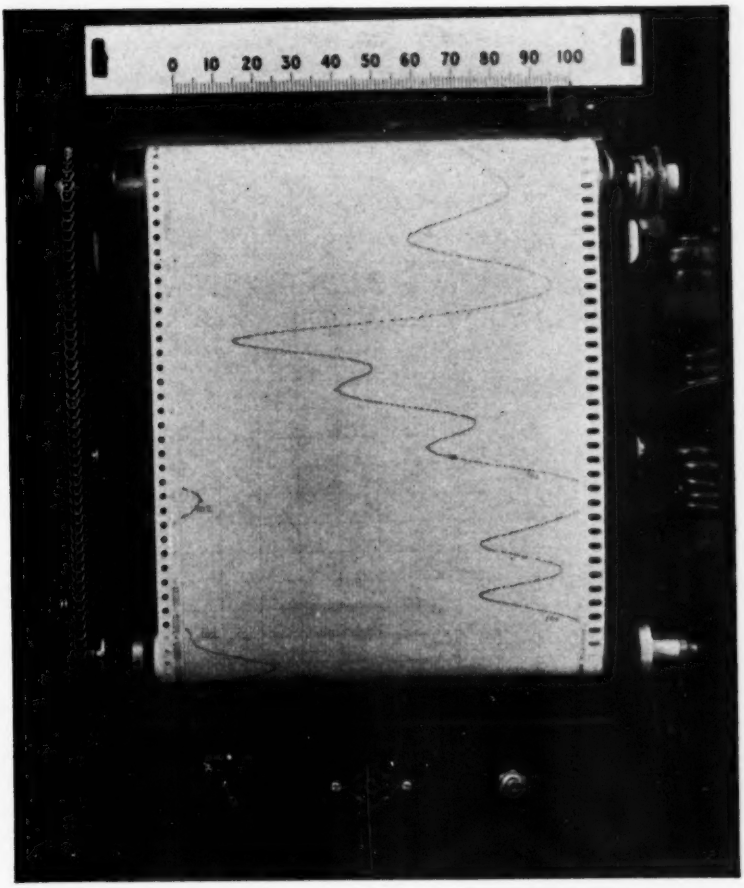
below, these sounds return to the surface of the sea at distances which may be many miles from the positions of the explosions. By measuring the speed with which these sounds travel, it is possible to learn such things as the depths to the layers constituting the ocean floor and whether these layers are composed of soft muds or sedimentary or igneous rocks. With explosions of a few hundred pounds in weight, we can trace the structure down to depths of many miles below the sea floor.

If two ships are available, then one can fire the explosions and the other receive the sounds travelling by the various paths through the ocean floor and through the water. In our work, however, we find it hard enough to obtain one ship, let alone

two and we, in Cambridge, have developed the method using sono-radio buoys. These buoys have hydrophones suspended below them which receive the sounds; these sounds are then radio-transmitted back to the ship from which the explosions are fired. The sounds from up to four buoys may be recorded simultaneously in the ship. Fig. 1 shows the system and various possible paths by which the sounds can travel in the sea floor.

Another device which from the sea surface tells us something of the rocks beneath the sea, is the towed magnetometer. The one which is most extensively used nowadays uses the principle of nuclear magnetic resonance; a description of this instrument is beyond the scope of this article but it obtains an absolute measurement of the Earth's total field every few seconds. Each measurement is recorded on punch tape or the measurements are collectively recorded on moving paper. (Plate II) The precision of measurement with our instrument is about 1 part in 50,000 of the total field; the maximum anomaly we have ever observed at sea is 1 part in 10 of the total field. Most ships are highly magnetic and measurements in the ship itself would not be possible with the requisite precision; the sensitive part of the instrument must therefore be towed astern at a distance roughly equal to twice the length of the ship. When this device passes over intrusions of, say, basaltic rocks through the sea floor, the Earth's field above them is disturbed and indications of the size and depth of these intrusions can be obtained by measuring the extent of this disturbance. The magnetometer is also very useful in finding the directions of the structural trends of the rocks forming the deep layers of the crust of the Earth beneath the sea.

As with the echo sounder, there is an enormous advantage in obtaining continuous profiles while under way. Another new device which also allows this is the ship-borne gravitymeter. This instrument is complicated by the stringent requirements of accuracy demanded in measuring gravity. The smallest change to be measured is about one millionth part (one milligal) of the total gravitational force at the surface of the Earth; the largest change being a few hundred times greater than this. These changes are caused by the variations in the density of the rocks forming the crust of the Earth. The instrument is essentially a highly damped spring balance with a mass on it; as the value of



A typical magnetometer record obtained between the deep Atlantic and Porcupine Bank. Nine hours' recording is shown; the ship's speed was eight knots. The chart width represents approximately 1 % of the Earth's total field.

PLATE II

gravity changes, so the deflection of the spring alters and its effective length is continuously recorded. The damping has to be very great since vertical accelerations of the ship may amount to 100,000 milligals and these must not cause the spring to change length. In order to eliminate the effects of horizontal accelerations, it is also necessary to maintain the axis of the in-

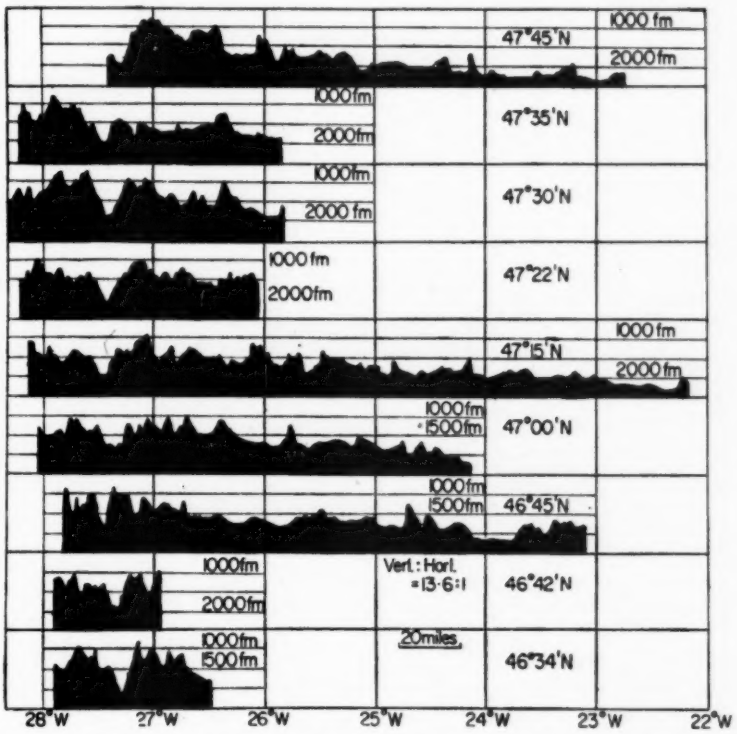


FIG. 2. East-West profiles from the eastern flanks to the centre of the Mid-Atlantic Ridge. The rift is conspicuous at the Western ends of all sections.

strument vertical to within a few minutes of arc; it is therefore mounted on a gyro-stabilised platform. This platform can deal with the ship's rolling up to between 20° to 30° on either side of the vertical and with pitching up to 10°.

Results

Some of the most interesting results obtained since the end of World War II have been with the study of deep-sea echo-sound-

ing records. Before the war practically no continuous profiles were in existence but after the war progressively more ships have been fitted with deep-sea sounders. It had long been realised that near the continental boundaries there were great areas of plains; how flat these plains were was not known. Now, however, we know that slopes of as little as 1 in 5,000 over distances of many tens or even hundreds of miles are commonplace on these so-called abyssal plains (Plate III). When the degree of flatness first became known, there was considerable speculation as to how such plains could be formed; now however, there is general agreement, as the result of extensive coring in these plains, that the material which has infilled the buried topography is largely of continental origin and that it has been deposited over these great areas by turbidity currents which sweep down the continental slopes loaded with clays and sands. These currents may travel over the plains for distances of up to 1000 miles before depositing their loads; even coarse sand grain particles are found at the extremities of the plains. When material finally starts settling out from the currents, the coarser fraction settles fastest. This results in the layer from any one turbidity current (which may be up to 10 ft. thick) being graded; the sand is at the bottom and the fine grained clays at the top.

Another feature of the Earth's surface which has only been discovered recently is the spectacular rift in the middle of the vast mountain range running down the middle of the Atlantic paralleling the continental boundaries. The origin of this rift is obscure but it has been suggested that it has been caused by tension in the Earth's crust due to an expansion in the Earth's diameter. Some cross sections of this rift are shown in Fig. 2.

Similar detail of the deep ocean trenches which are most commonly found on the continental boundary of the Western Pacific has been obtained by echo sounding. These trenches not only provide the greatest oceanic depths in the world (more than 35,000 ft.) but also are characterised by having a large mass deficit beneath them. This is indicated by the gravitymeter measuring several hundred milligals less over the axes of the trenches than over normal oceanic areas. In these normal areas the value of gravity is such that the relatively light sea water must be compensated for by more dense rocks closer to the surface than is

A. The appearance of the sea floor on an abyssal plain of 2900 fathoms depth. The photograph shows the typical reworked surface where the deposition rate is very low. The origin of this track is unknown. The area of the picture is approximately $4\frac{1}{2}$ ft. square.

B. Bedrock emerging from the sediments on the flanks of a seamount in a water depth of 1600 fathoms. The area is approximately 5 ft. by 8 ft. Both these photographs were obtained by Dr. A. S. Laughton of the National Institute of Oceanography.

PLATE III

usual under continental areas. In contrast, the trenches are regions where forces are holding down the sea floor which otherwise would, because of the plasticity of all rocks, gradually return to a normal oceanic depth.

This closeness of these dense rocks to the surface has been indicated by seismic methods of exploration and the structural differences between the oceans and continents has, in general, the form shown in Fig. 3. On the surface of the ocean floor there are several hundred feet of soft sediment, either clays or the shells of minute surface living organisms. Beneath this is "Layer 2" which might represent consolidated sediments or

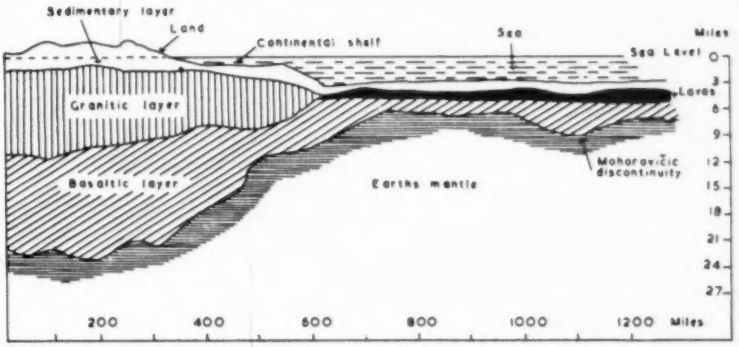


FIG. 3. A generalised section through the crust of the Earth from continental out to oceanic regions.

basaltic lavas. (Preliminary "Mohole" results indicate the latter to exist beneath the sediments in at least one place).

Layer 2 overlies what we believe to be basalt which possibly also exists at the base of the continental crust. Beneath this layer the Mohorovicic Discontinuity (or Moho) is reached and below this discontinuity the mantle rocks of the Earth extend to the liquid Earth's core which is reached about half way to the centre. Beneath typical oceanic areas the distance from sea level to the Moho is about 33,000 ft.; beneath the continents it is usually at about three times this depth.

These are some of the details of Earth structure which our new methods of exploration can reveal. Much remains to be done; indeed as beneath the continents, so beneath the sea

floor, there are still many enigmas which we cannot resolve without much more patient, slow, and sometimes frustrating work, using existing and hitherto undeveloped techniques. So little is at present known about the sea floor that exploration is challenging and the results exciting.

EXHIBITS IN THE LIBRARY

A display illustrating methods of exploring the ocean floor, including (*a*) demonstration of a towed magnetometer and related equipment; (*b*) seismic refraction shooting equipment; (*c*) diagrams and pictures illustrating gravity recording at sea and a deep-sea corer; (*d*) specimens of rocks from the bottom of the sea. Arranged by *Dr. M. N. Hill and the Department of Geodesy and Geophysics, University of Cambridge.*

WOODHULL LECTURE

PRINCIPLE AND PARADOX IN EUGENICS

By P. B. MEDAWAR, C.B.E., M.A., D.Sc., F.R.S.
Jodrell Professor of Zoology and Comparative Anatomy, University College, London

Weekly Evening Meeting, Friday 27th January, 1961

Brigadier H. E. Hopthrow, C.B.E., M.I.Mech.E.
Secretary and Vice-President, in the Chair

THIS is the sixth of the annual Woodhull Lectures, and it follows the tradition of its predecessors in being concerned, as Miss Woodhull would have wished, with the problems of Eugenics—with the steps we must take to promote and safeguard the genetic welfare of human beings in the years that lie ahead. Let me begin by defining eugenics in the language of its founder, Sir Francis Galton; the quotations I give you were chosen by his biographer, Dr. C. P. Blacker, to represent the very gist of his thought. "Eugenics," said Galton, "is the science which deals with all the influences that improve the inborn qualities of a race; also with those that develop them to the utmost advantage." So he wrote in 1904. And again, in 1908, he wrote: "Man is gifted with pity and other kindly feelings; he has also the power of preventing many kinds of suffering. I conceive it to fall well within his province to replace natural selection by other processes that are more merciful and not less effective. That is precisely the aim of eugenics."

These are honourable and humane ambitions; what prevents our trying to realize them forthwith? A first difficulty is that *two* ambitions are expressed by these quotations; they may not be fully reconcilable, and the policies that serve one may not necessarily serve the other. Galton declares his concern for the welfare of the race, of the human species, of mankind considered collectively; but, being a humane man, he is also deeply concerned with the happiness and welfare of the individual. At first sight it seems obvious that what is best for each member of the population considered individually must be best for the race considered as a whole. This is not necessarily so. It might be so if we lived, and had always lived, in a uniform and unchanging environment,

in which natural selection could, as it were, devise some *one* best genetic solution of the problem of remaining alive and perpetuating our kind. In real life, however, the environment changes from time to time and from place to place, and it is therefore unconditionally necessary that a free-living population should always contain at least some members that can cope with new environments, new climates, new enemies and new infections. This puts a certain high premium on genetic inequality or diversity. Some geneticists believe that free-living outbreeding populations such as our own are armed by an inbuilt genetic device which *enforces* genetic diversity, which makes sure that not all its individual members are genetically identical. This device is one in which predominantly hybrid (or rather, heterozygous) animals leave the most offspring, i.e. are the fittest in the Darwinian sense. Genetical diversity is ensured by the fact that hybrid animals do not "breed true": their progeny differ from themselves and from one another. So far as present evidence goes (though some geneticists dispute it) it does seem true to say that the genetic make-up of outbreeding populations is a compromise between getting the best performance out of particular individuals and at the same time making sure that the population contains a great variety of individuals of different genetic types. It is a compromise between adaptedness and adaptability. If this interpretation is true, it implies that the genetical system of human beings cannot be reconciled with the Utopian conception of the older eugenicists—with the belief that, by adopting one genetic policy or another, it is in principle possible to arrive at a uniform population of uniformly excellent individuals who "breed true", i.e. whose progeny have the same genetic make-up as themselves.

You may argue that livestock breeders have already achieved this very thing in establishing "pure breeds" that answer their preconceived requirements; that they have mastered the arts of applied genetics, and that it is therefore open to us, in principle, to apply their knowledge to man. Unfortunately, they have done nothing of the kind. Livestock breeding is still a matter of empiricism and opportunism. One great branch of the livestock industry is moving rapidly towards the policy of crossbreeding—a frank abandonment of the Utopian goal represented by the pure-breeding animal which in itself embodies the characteristics

that the breeder seeks. We are not nearly as well informed about evolutionary genetics as we may like to think. Consider for a moment Dr. Evelyn Hutchinson's amusing paradox about the genetical background of the paraphilias—of abnormal sexual preferences or tastes, homosexuality for example. Let us assume that paraphilic tendencies are deeply grounded in the genetic constitution; and let us also assume (what is very likely, though it has not been proved) that on the average paraphilics are less fertile than normal people—less fertile in the demographer's sense of leaving fewer children, and of making, therefore, a smaller contribution to the population of the future. If these assumptions are true, then natural selection should act in such a way as to eradicate the genetic constitutions that predispose towards paraphilia. Yet, very evidently, it has not done so. Either, then, we simply do not understand the pattern of the inheritance of sexual tastes; or, alternatively, we must conclude that these tastes are not under any significant degree of genetical control, in which case the influence of genetic make up on our behaviour has been greatly exaggerated.

In my opinion current genetical knowledge will simply not sustain a Utopian genetics of race, of species, or of man in the abstract. What we must attempt to do is to replace this grandiose and heroic eugenics by a much more humble policy which, by a piece of gross plagiarism from Professor Karl Popper, I propose to call "piecemeal genetical engineering"—in the words of William Blake, the policy of "doing good in minute particulars".

Before I turn to consider the ways in which the more humble policy I advocate might be put into effect, let us consider the principles that justify our belief in eugenics as a humane, rational and practicable enterprise. The first is that *defects* of the genetic constitution have a simple and particular basis, though they do not always have simple and particular consequences. This is self-evident. All geneticists believe that "fitness" in its most general sense depends upon a nicely balanced coordination and interaction of particular genetic factors, itself the laborious product of evolutionary adjustment. It is inconceivable, indeed self-contradictory, that an animal should evolve into the possession of some complex pattern of cooperation between genes that put it at a disadvantage, or made it inefficient or unfit. Breakdowns or

ineptitudes of the genetic apparatus are therefore adventitious in character, or, as philosophers would say, accidental. Mutations and chromosomal accidents may be so described, and so also the unlucky genetic conjunctions that bring together a pair of harmful recessive genes within one individual. The point, then, is that many genetic defects are, in principle, *manageable;* that even when the somatic consequences of a particular genetic defect are manifold and widespread, we can still in principle entertain the possibility of prevention or of cure.

It is a lay superstition that congenital afflictions are incurable. The very word "congenital" has a dire sound, because it is used only in a pejorative sense: one hears of congenital imbeciles but never of congenital geniuses. It is quite untrue to say that the congenital diseases cannot be cured. Phenylketonuria and galactosaemia will be cured in the foreseeable future. It is true that if one cures a condition like phenylketonuria by making certain adjustments of diet, a genetical defect remains; but the important thing to remember is that the genetical defect that remains is not phenylketonuria—it is a genetic disability that debars its victims from eating what they choose. In other words, it is a genetical disability which leads to a restriction of freedom. Certainly it is worse than the sort of disability which obliges quite a number of unfortunate people to wear spectacles, but not so much worse that we should be content to see its victims die.

There are some inborn genetical disorders for which it is very difficult to envisage the possibility of a cure. The most important of them is mongolism, an important source of human wastage and distress. In 1959 three French geneticists demonstrated that mongolism is the result of a genetical accident involving a whole chromosome—the triplication of a chromosome which in normal individuals exists only in duplicate. Fortunately, most forms of mongolism do not raise a eugenical problem: most mongols are either actually or effectively infertile. Here I think one must take comfort from the fact that there are certain rather widely pervasive demographic tendencies which in themselves will reduce the incidence of mongolism. One of them is the tendency, already apparent in the United States and the United Kingdom, for married couples to complete their families a good deal earlier in married life than they would have done fifty or a hundred

years ago. The frequency of mongolism in children rises very sharply with the age of their mothers, and the tendency towards an early completion of families is therefore bound to lower its incidence.

When we turn to the particular forms a policy of "piecemeal genetical engineering" might take, I think we shall find a great measure of agreement among geneticists, for the rancour and bitterness that has disfigured the history of eugenics has arisen mainly out of matters of grand genetic policy.

Consider first what are loosely called the "dominant" inborn diseases, diseases attributable to harmful genes which have been inherited from only one parent, or have arisen by mutation in only one gamete. There must be many such afflictions, and they may account for much of the unexplained balance of human mortality before birth. The most merciful, paradoxically enough, are those so rapid and devastating in their effects that they destroy the embryo early in pre-natal life, perhaps shortly after conception. They may cause no more distress than an early miscarriage or a missed menstrual period. Afflictions of this kind are self-limiting and raise no great eugenic problem; the genes responsible for them must have arisen by mutation, and disappear with their possessors. But grave eugenic problems are raised by dominant afflictions which, like achondroplasia, cause grave damage short of death, or which do not make themselves apparent until later on in life. Among the latter are Huntington's chorea and familial intestinal polyposis, leading to cancer. The average age of incidence of both is about 35, so that parents who are to be victims of the disease can have children before it becomes manifest. I think most geneticists would agree that the humane and rational policy here is to warn the future victims of these diseases, if they can be identified, of the consequences of their having children; to warn them that, on the average, half of any children they may have will be afflicted as they were themselves. No humane parent would have children at such a high risk of their being exposed to acute physical and moral distress. When one of the potential parents is himself or herself the child of a victim of the disease, the problem will be to identify the disease early enough in life for the warning to be effective. The task will be much more difficult, though not insuperable, when the offen-

ding gene has arisen by mutation, so that there is no family history to put the parents on their guard. But if all the future victims of Huntington's chorea and familial intestinal polyposis could be identified in good time, and if all foreswore having children, then the incidence of the disease would be reduced to the very low rate at which it is introduced into the community by mutation.

As Professor Haldane pointed out in an earlier Woodhull Lecture, much the same argument applies, *mutatis mutandis*, to sex-linked recessive diseases like haemophilia. With very rare exceptions, only males are afflicted. A male parent cannot transmit the gene responsible for it to his sons, but he transmits it to all his daughters, with the effect that, on the average, half his daughters' sons will be afflicted by haemophilia and half his daughters' daughters, like their mother, will be carriers. Here too, a victim of haemophilia should be warned of the consequences of his having children. Where there is a history of haemophilia in a family, it would of course be of the utmost advantage to identify those women who are carriers of the gene, but this is not yet possible.

The gravest eugenical problems arise over the "inherited" diseases that are collectively the most numerous: the so-called "recessive diseases", produced by genes that must be inherited from *both* parents (or which must be carried by *both* gametes) if they are to exercise a damaging effect. In such a context the parents are carriers of (i.e. are heterozygous for) the gene, though they are not harmed by its presence. Among recessive diseases are fibrocystic disease of the pancreas, which is said to afflict approximately one child in 2500 (a very high figure for any one such disease); phenylketonuria, which leads to a form of mental deficiency, the causes of which have been biochemically identified; and another biochemical disease with widespread consequences, galactosaemia.

What is to be done here? The policy of sterilizing the afflicted has been discredited, for those in whom the disease makes itself manifest are a very small fraction of those who, in the genetical sense, "carry" it. To discourage childbearing by the carriers of a harmful recessive genes would be to commit racial suicide, for all of us carry such genes. Some think that each one of us may

carry as many as eight. A third policy, advocated by Professor Haldane, depends for its success on being able to identify the carriers of harmful recessive genes, usually by their possession of some very mild and in itself trivial disorder biochemically akin to the full recessive form of the disease. If the carriers of single particular harmful recessive genes could be identified, they should be warned of the consequences of marrying *each other* and having children—for, on the average, one quarter of their children will be victims of the disease, and one half carriers. The proportion of marriages "contraindicated" by this policy would not be large: if the incidence of the disease were one in 40,000 (which is high), only about one marriage in 10,000 would be a marriage between its carriers, to be advised against for that reason. In these days of blood grouping, very few people would object to the performance of simple biochemical tests that might do so much to secure the welfare of their future children, and there will be no insuperable difficulty in applying such tests when a family history gives grounds for suspecting that a future parent may carry a particular recessive gene.

The wholesale application of such a policy (when we are technically equipped to put it into practice) would reduce the overt incidence of particular recessive diseases to a very low level indeed. This would be a *symptomatic* cure of the eugenic problem. If our ambition is to eradicate the disease, this policy will put it in our power to do so. There is only this danger to be considered. The frequency of the occurence of a lethal or incapacitating recessive disease represents an equilibrium between the rate at which the gene responsible for it is introduced into the population by mutation and the rate at which the gene is lost by the death or infertility of those in whom its effects appear. This is simply natural selection at work. It is to be assumed that, where carriers of particular recessive genes have been dissuaded from marrying each other, they will marry other, normal people, and have children by them. The gene will therefore be propagated in the population, and natural selection will be unable to prevent its slow but progressive increase by mutation—for by preventing the genetic conjunction that brings two such genes together, we are denying natural selection its opportunity to reduce their number. In the very long run the carriers of particular genes

must have children by each other if they are to have children at all.

This is a danger of principle only, and I use it only to illustrate the antithesis between "symptomatic" eugenics and eugenics of a more radical kind. It does however point the moral that ultimately, in a humane society, the only formula that will meet the problem of damaging recessive diseases is "study and cure".

The problems first raised by Sir Francis Galton are becoming slowly and progressively more urgent. I say "slowly" because, after all, the unit of the time scale of human evolution is a generation of some 20 or 30 years. This is a most reassuring fact, because (if you will forgive my comparing two obviously incommensurable quantities) the rate of accession of knowledge of human genetics is enormously greater than the rate of human evolution. Not much human evolution has happened in the five years since Professor Haldane gave the first Woodhull Lecture. In that period the causes of at least one human polymorphism, that responsible for sickle cell disease, have been elucidated. Great progress has been made in identifying the carriers of harmful recessive genes. A new class of inborn disorders has been disclosed by the study of human chromosomes. Much has been learnt about the genetical effects of radiation on whole animals or cells. Progress has been made with the study of the most difficult problems of genetics, the inheritance of differences controlled not by one or a few, but by a multitude of cooperating and interacting genes. And it is important to remember, that on this time scale, the proposals that we may make today about safeguarding the genetic welfare of human beings are not binding upon us in perpetuity; they are not irreversible, and if they turn out to be faulty they can be revised.

I say "progressively" because, of those children who still die in infancy or are afflicted by disease, a steadily increased proportion is afflicted by "inborn" disorders. I emphasize that it is a matter of proportion, not of absolute numbers: the inborn disorders take a larger toll because the infective and metabolic diseases take a lesser. In any event, the generalization is true only of advanced industrial countries. In the world as a whole, the inherited disorders are still of minor importance. Some 250 million people are said to be afflicted by malaria in the world today, of whom some 2½ million die each year; and no-one knows

the number of deaths that can be attributed directly or indirectly to nutritional diseases, above all to protein-deficiency diseases.

In summary, then, I have drawn a distinction between, on the one hand, a heroic eugenics whose ultimate ambition is to reshape the genetic constitution of mankind; and, on the other hand, a "piecemeal genetical engineering" dedicated to the welfare of individual men and women, and content with the policy of doing good in minute particulars. I propose that we forswear the heroic eugenics and blood and soil and species and race, since it may be faulty in principle, and since neither our knowledge nor our humane inclinations will run to it. I began with two quotations from Galton: let me enlist him as my ally by a third:

"It is above all needful for the successful progress of eugenics that its advocates should move discreetly and claim no more efficacy on its behalf than the future will confirm; otherwise a reaction will be invited."

EXHIBITS IN THE LIBRARY

(*a*) A selection of pictures of normal and abnormal human chromosomes, lent by *Dr. C. E. Ford, M.R.C. Radiobiological Research Unit, Harwell.*

(*b*) Historical and modern Stud Books, lent by *Messrs. Weatherby & Sons.*

(*c*) A selection of new and recent publications, lent by *Macmillan & Co., Ltd.*

DIAMONDS

By SIR GORDON SUTHERLAND, Ph.D., Sc.D., F.R.S.
Director, National Physical Laboratory

Weekly Evening Meeting, Friday 10th February, 1961

E. R. Davies, O.B.E., B.Sc., F.Inst.P.
Vice-President, in the Chair

OF all the naturally occurring inorganic substances, diamonds have for long exerted a peculiar fascination on men and women. Thousands of years ago their extreme hardness led to their use as charms by men; their use as gem stones by women (not surprisingly, it is said that the first was a French woman) appears to date back to about 1400 A.D., when men discovered the art of cutting a diamond in such a way as to exhibit brilliance. The exploitation of their hardness for industrial purposes is over 100 years old and nowadays there is scarely any manufactured article which does not owe something to industrial diamonds, directly or indirectly, at some stage of its production. Diamonds have been of interest to scientists for over 100 years, but during the past ten or fifteen years it has been realized that they present a surprisingly large number of extremely interesting and difficult problems to the physicist.

The first discourse on diamonds to this Institution was given by N. S. Maskelyne just over 100 years ago (23rd February 1860); the second by Sir William Crookes in the year of Queen Victoria's Diamond Jubilee (16th June, 1897); the third by Dame Kathleen Lonsdale in 1944; and the fourth by Professor Harold Urey in 1956. The fact that the interval between discourses on diamond is decreasing so rapidly is an indication of the growing interest of scientists in diamond.

I do not know which scientist first interested himself in diamond, but there is on record a statement by Newton, viz. "O Diamond, Diamond, thou little knowest the mischief (thou hast) done". However, Newton was not apostrophizing the gem stone but talking to his dog (called Diamond) which had upset a candle and set fire to many valuable scientific manuscripts!

The earliest records of diamonds as gem stones and ornaments came from India, but as you all know, it was the discovery of vast quantities of diamonds in South Africa around 1870 which made

diamonds familiar objects all over the civilized world. The original discovery was made by the children of a Boer farmer, Jacobs, in 1866, but not much attention was paid to it as intensive searches for more near the Vaal were fruitless. However, in 1868 a Griqua witch doctor named Booi picked up an 83½ carat diamond near the Orange river. This stone eventually became the Star of South Africa (46½ carat). This stimulated more extensive searches and by July 1871 the now famous Du Toits Pan,

FIG. 1. The arrangement of the carbon atoms in diamond.

De Beers and Kimberly fields had all been discovered. Diamonds have also been found in economic quantities in South America (Brazil, British Guiana and Venezuela), Russia and in other parts of Africa, e.g., Gold Coast, Sierra Leone, Tanganyika and most important of all, the Congo, from which comes about two-thirds of the annual world production (21 m. carats) of industrial diamonds. Lastly, diamonds have been found in meteorites, the most famous being the Canyon Diabolo meteorite in Arizona.

Diamonds are first sorted into industrial grade and potential

gem stones. The former are poorly coloured, flawed, mis-shapen and often no larger than a grain of sand. The potential gem stones are usually octahedral or flat (called portrait type because they were originally used in India to form the glass over miniature portraits painted on ivory to be worn on the bosom). The portrait stone is really a malformed octahedron in which two of the opposite faces have grown much more rapidly than the other faces. Diamonds are also found in the cubic and dodecahedral forms. As you will see from the exhibit in the Library, diamonds can occur in a variety of colours, e.g., green, pink, pale blue and brown.

Since most people are unfamiliar with the carat, I might remind you that a ten carat brilliant is about the size of a sixpence and weighs approximately 1/15 oz. The largest diamond ever found was the Cullinan, of 3025 carats or about 1½ lbs.

RELATION OF THE ATOMIC STRUCTURE TO THE COMMON PHYSICAL PROPERTIES

The atomic structure of diamond was first elucidated by the Braggs whose X-ray diffraction work proved that the carbon atoms form a regular lattice in which each carbon atom is at the centre of a tetrahedron formed by the next nearest four carbon atoms (Fig. 1). This structure might have been anticipated from the chemical evidence on the tetravalency of carbon if one assumes that all of the chemical bonds in diamond are identical single C-C bonds. This structure also satisfies the crystallographer since all of the carbon atoms lie on two interpenetrating cubic lattices, thus accounting for the fact that diamonds are found to crystallize as cubes, octahedra and dodecahedra.

Hardness

In order to deform or break a diamond it is therefore necessary to break a large number of quite strong primary chemical bonds, i.e., a perfect diamond can be regarded as an enormous single polymer molecule. Now, the strength and toughness of any polymeric material (such as polythene, nylon or teflon) is known to depend on the length of the polymer molecule, but even more on the degree of cross-linking between the individual polymer molecules. Thus nylon is tougher and harder than polythene

because the nylon molecules are cross-linked by hydrogen bonds between the CO and NH groups whereas no such interaction occurs between the CH groups in polythene. Diamond can be re-

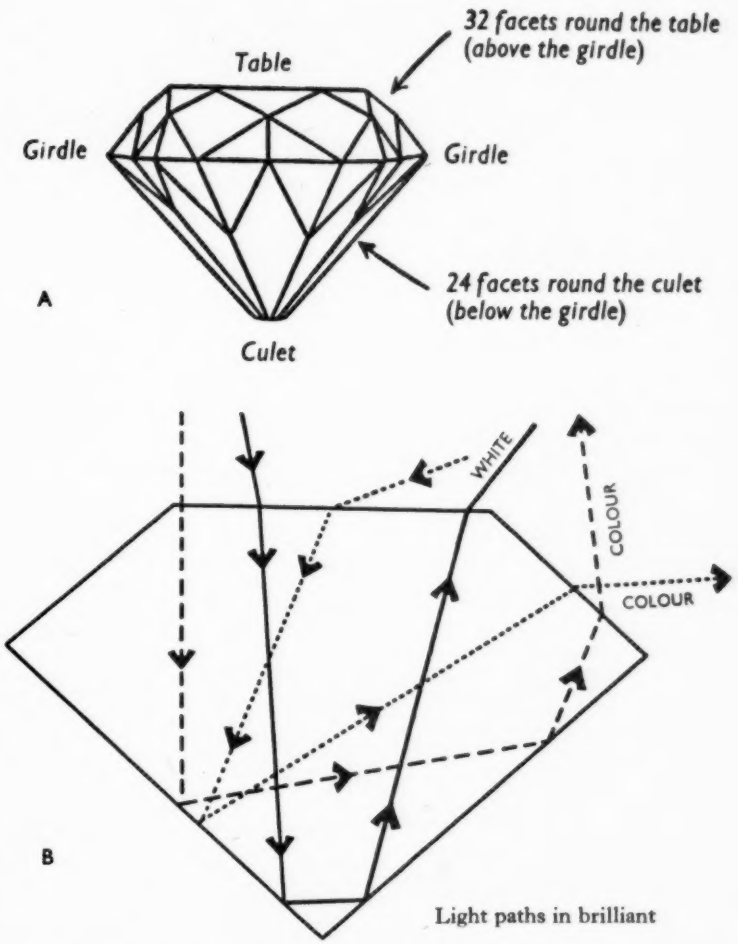


FIG. 2. (A) A diamond cut as a "brilliant". (B) Paths of typical light rays in a diamond brilliant.

garded as a polymer which is completely cross-linked by strong primary chemical bonds. It is harder than inorganic minerals (such as ruby and sapphire) where the forces holding the crystal

together are ionic and weaker than the covalent chemical bonds in diamond.

Incidentally, the fact that diamond is really one huge molecule also accounts for its extremely low coefficient of thermal expansion and its very high thermal conductivity.

Brilliance

The extraordinary brilliance or "fire" of cut diamonds is well known to be due to their exceptionally high refractive index (2.4) leading to an unusually low angle for total internal reflection. This is exploited in the way in which a brilliant is cut so that almost all the light incident on it is reflected back after dispersion into the various colours of the rainbow (Fig. 2A and B).

The very high refractive index can also be accounted for in a general way from the Bragg structure, from which the value for the density of 3.52 which may be predicted (cf. the experimentally observed values of from 3.520 to 3.531.) Now, the density and refractive index (n) of any chemical compound are related to the molecular structure by the Lorentz-Lorenz formula

$$[R] = \frac{n^2-1}{n^2+1}\,\frac{M}{\rho}$$

where [R] is the molecular refractivity, i.e., the sum of the refractivities of the individual atoms, M is the molecular weight and ρ is the density. This formula, which is strictly applicable to pure liquids, can be shown to predict a very high (but not very accurate) value for the refractive index of diamond.

Abrasion

It is found that the faces of diamonds are not equally hard, and also that the hardness varies strongly along different directions in any one face. This is important in the use of diamonds for grinding. The abrasion resistance is greatest for the (111) octahedral plane, less for the hardest direction of the (100) cubic plane, less again for the softest direction of this plane, and least for a certain direction in the (110) dodecahedral plane. This accounts for the persistence and excellence of natural octahedral diamonds. The explanation of the relative hardness of the different planes is not known. It is probably due to differences in microcrystal-

line structure arising in the growth of the diamond. It has been reported that some synthetic diamonds are better abrasives than natural diamonds, but it would appear that this is true only when applied as an abrasive in grinding wheels under special very light load conditions.

Cleavage

Diamonds cleave easily along the (111) octahedral face. This is exploited in the cutting of diamonds as gem stones. Again, the explanation is not certain, but probably this is an intrinsic property arising from the atomic arrangement.

Difference between Diamond and Graphite

Why is diamond so different in its physical properties from graphite since each consists purely of carbon? The answer is that the arrangement of the carbon atoms in graphite is entirely different from that found in diamond. In graphite the carbon atoms are arrayed in sheets consisting of linked hexagons (Fig. 3). This explains why graphite flakes so easily (cleaving parallel to the planes of atoms) and makes a good lubricant since slipping occurs easily on these same planes. The blackness of graphite, as opposed to the transparency of diamond, is a consequence of the peculiar electronic structure surrounding the carbon atoms. The carbon atom has four electrons which can take part in interatomic chemical bonds. In diamond, each one of these electrons forms a single bond with a neighbouring carbon atom. In graphite, three of the four electrons form similar bonds in the plane of linked hexagons, but the fourth electron may be regarded as spending half its time above the plane and half its time below the plane of carbon atoms. Such electrons are often referred to as π electrons as opposed to the σ electrons in the normal single bonds. Molecules in which the absorption spectrum is determined by the excitation of σ electrons in carbon atoms show no absorption of visible light and only begin to absorb in the far ultra-violet (around 2200 Å). Molecules in which the absorption spectrum is determined by the excitation of π electrons absorb in the near ultra-violet (3000 Å) and this absorption moves into the visible when there are many π electrons in a closely coordinated array, e.g., the cyanin dyes. It is therefore

not surprising that graphite, which is the extremest form of such a conjugated array, absorbs all visible radiation and so appears black.

THE TWO TYPES OF DIAMOND

So far we have been able to explain most of the remarkable physical properties of diamond in terms of the Bragg structure. However, about 25 years ago the late Sir Robert Robertson, the

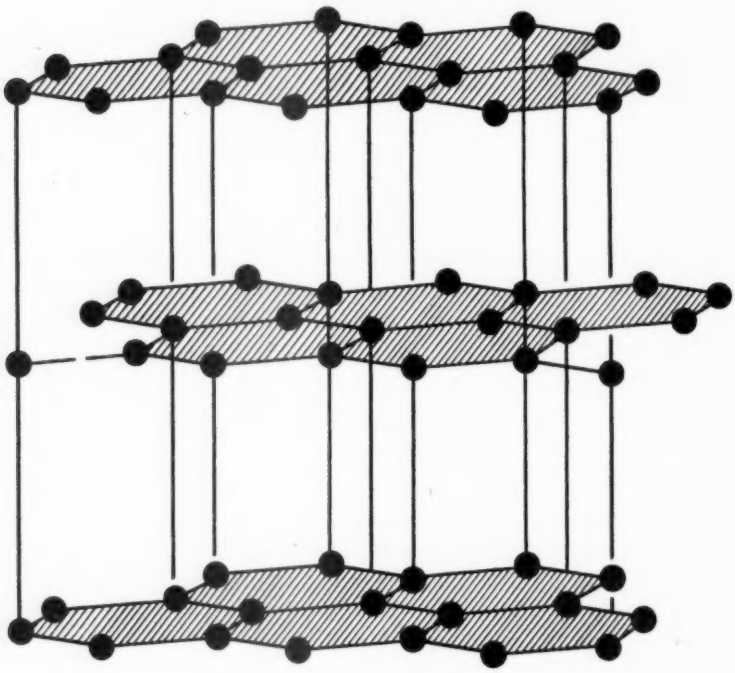


FIG. 3. The arrangement of the carbon atoms in graphite.

late Sir John Fox and Dr. A. E. Martin[1] made the first systematic investigation of the infra-red and ultra-violet spectra of diamonds. About six out of several hundred were peculiar in showing greater transparency in the infra-red and ultra-violet than all the others (Fig. 4). They called the commoner variety Type I and the rarer variety Type II, but were unable to offer any explanation of the marked differences between them. Shortly afterwards, Dame Kathleen Lonsdale showed that many Type I

diamonds showed extra streaks in their X-ray pattern whereas Type II diamonds gave a perfectly normal pattern. Later, we were able to show that there are not two distinct spectral types so much as an extraordinary variety and range of which Type I and Type II form extreme limits. It was at first thought that Type II are very rare, small and imperfect in crystalline form. It now seems that although Type II are comparatively rare (thus, De Beers give a rough estimate of 1 in 2000) some of the largest natural diamonds discovered in the past 10 years have turned out to be Type II (e.g., the Winston diamond). This is especially true of the "blues" which are occasionally found in the Premier Mine and in the Jagersfontein Mine.

It was pointed out by Blackwell, Simeral and myself[2] that the Type II diamond has all the spectroscopic characteristics which would be predicted on the Bragg structure, whereas Type I diamonds show additional absorption and additional streaks in their X-ray patterns which can formally be accounted for by the presence of impurity centres in the crystal. These centres might either be foreign atoms or vacancies or carbon atoms with an anomalous electronic configuration. Within the past three years, scientists at the Bell Telephone Laboratories in the USA have proved that many Type I diamonds contain up to 0.25% of nitrogen[3]. Just how this nitrogen is embedded in the lattice is still not yet clear. Experiments on electron spin resonance indicate that only about 10% of the nitrogen is in chemical combination with the carbon. Whether there are other impurities as well as nitrogen which would account for some of the features of Type I diamonds is not yet certain.

However, it is interesting that the majority of diamonds, long regarded as one of Nature's most perfect structures, are far from being either pure or perfect at the atomic level. By "the majority of diamonds" I mean all Type I diamonds, but I should add that there is a small category of Type II diamonds which, in all probability, also contain impurities dissolved in the lattice, though their minor element concentration has been found to be smaller again than that of all other diamonds*. These are known as Type II b diamonds. They were first noticed by Blackwell, who

*Private communication from the Diamond Research Laboratory, Johannesburg.

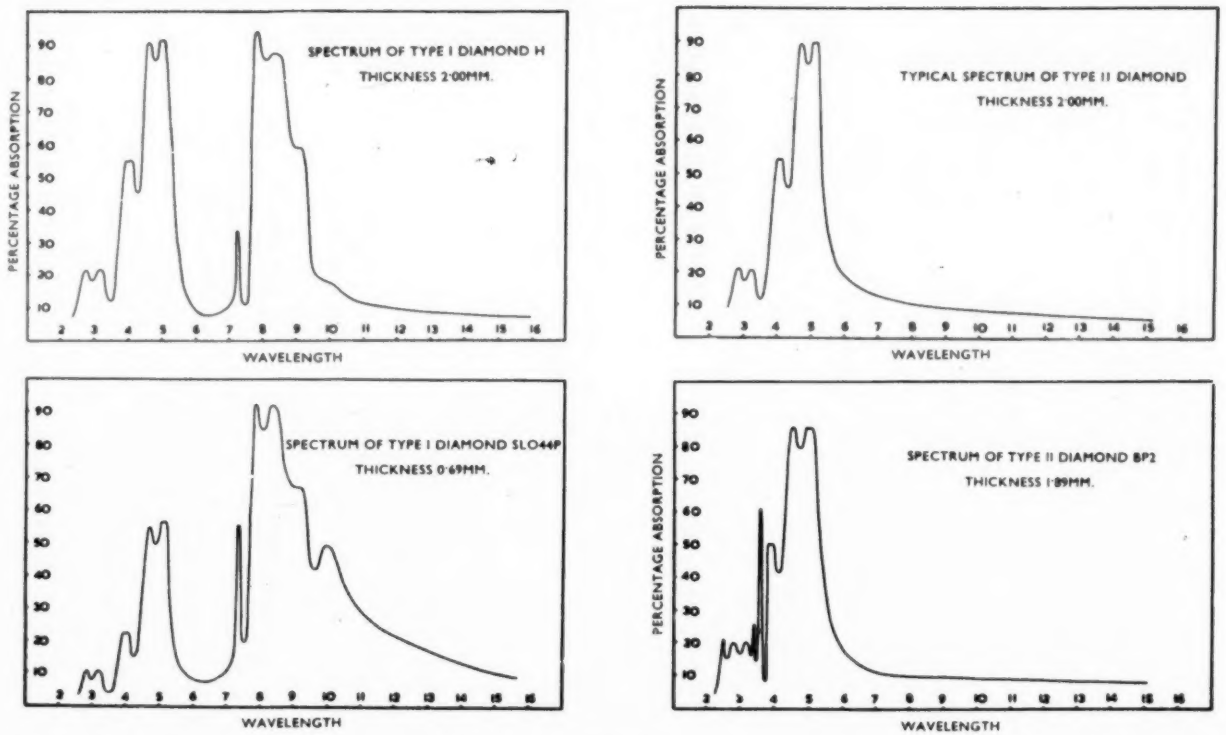


FIG. 4. Infra red absorption spectra of type I and type II diamonds. The spectrum on the lower right is that of a type IIb diamond.

found that they possessed a few weak bands near 3.4 μ in the infra-red. However, the really remarkable property of these diamonds was discovered by Custers[4], who showed that they were semi-conductors. Here I have one of them, across which I put about 100 volts from a dry battery and on the diamond I have put a little wax. You will see that in about a minute the diamond becomes so hot that the wax melts. Indeed, unless one puts a limiting resistance in series with it, the diamond can become red hot. Some impurity must be present to produce the energy levels which cause the diamond to be a semi-conductor. The impurity has not yet been identified.

Attempts have been made to change Type II into Type I diamonds by irradiation with neutrons, high energy electrons, protons and deuterons and with X-ray and γ rays. Only very limited success has been achieved in producing the characteristic infra-red absorption of the Type I diamond but marked changes are produced in the ultra-violet and even in the visible part of the spectrum. It is possible to produce green diamonds. When neutrons are used as radiation, the whole volume of the diamond is coloured a lighter or a darker green, dependent on the dose, and most of this colour can be removed by heating. However, the colour is only "skin-deep" when irradiating with charged particles such as electrons, and the resulting colour also depends on the energy of the bombarding particles though it is mostly greenish or bluish green. Many interesting phenomena have been discovered by work of this kind at Reading University and London University, but it is impossible to give even a brief description of them here because of their complexity.

SYNTHETIC DIAMONDS

Around the turn of the century many attempts were made to synthesise diamonds from solutions of carbon, paraffin or other hydrocarbons in iron and other metals. Using his electric furnace Moissan, in France, was convinced that he had been successful. In this country Crookes and Hannay also claimed success. Indeed, in his lecture at the Royal Institution, to which I have referred earlier, Crookes made the following statement, "I now proceed to manufacture a diamond before your eyes". None of these claims has ever been substantiated. It is very odd that no

specimens of these synthetic diamonds appear to have been preserved except a few made by Hannay which are in the British Museum. The X-ray diffraction pattern of these was investigated several years ago by Dame Kathleen Lonsdale[5], who found them to be Type II diamonds. Considering the rarity of

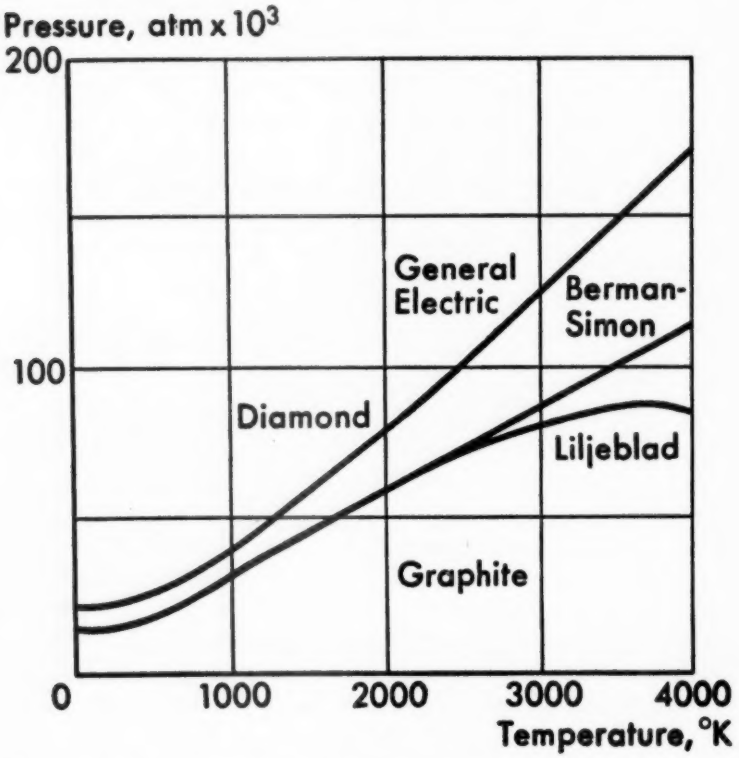


FIG. 5. Graphite—diamond equilibrium curves according to various authorities.

Type II diamonds, this seemed strong presumptive evidence that the Hannay diamonds had been synthesised and not put into the reaction vessel by an unscrupulous experimenter. According to information obtained from the Adamant Research Laboratory in Johannesburg, synthetic diamonds may be of either one of the two well-known Types I and II respectively, and of the few examined (8 in number) six were transparent to below 2302Å

whereas two transmitted only as far as about 2893Å. Against this must be placed the fact that the average synthetic diamond is coloured and much less perfect in appearance than the Hannay diamonds.

Why are diamonds so difficult to synthesise? The reason should be clear from Fig. 5 showing the phase diagram for equilibrium between diamond and graphite under varying conditions of temperature and pressure. You will see that at ordinary temperatures and pressures graphite is the stable form of carbon. However, if carbon atoms can be got into solution (iron dissolves 9% of carbon at 2200°C) then above 70,000 atmospheres and 2000°C it should precipitate as diamond and remain as diamond if the temperature is lowered before the high pressure is gradually released.

During the past ten years or so, controlled experiments were made in the USA and Sweden to produce the required temperatures and pressures. The General Electric Co. of USA announced success in 1955.[6] Immediately thereafter Liander and Lundblad[7] of the ASEA Company in Sweden disclosed that they had also made some very small diamonds as early as 1953. In the past two years synthetic diamonds have been made by De Beers in South Africa and in other laboratories in the USA. In the past year we have set up high pressure equipment at the National Physical Laboratory, and last week produced a few very small diamonds.[8]

At present, synthetic diamonds are small (the largest so far produced by the General Electric Company is about 1 carat, but it has been stated by the G.E. themselves that these larger ones are most imperfect and of little or no commercial value) and are suitable only for industrial use. It has been reported that the smaller synthetic diamonds, up to sizes of the order of 0.3 mm, are superior in hardness to natural diamonds, but this applies to certain applications only as was stated above. The total production of synthetic diamonds has been estimated to be about 3 million carats per annum, i.e., approximately 10% of the total annual world production.

GROWTH AND GENESIS OF DIAMONDS

When the surface of a good natural octahedral diamond is examined under the microscope, it is usually found to be covered

with tiny triangular pits known as "trigons" (Plate I). A typical "trigon" might have a side 0.05 mm long and a depth of .0005 mm but many are smaller and relatively shallower (as small as 10 A U). There has been a good deal of controversy over the origin of these "trigons", in particular, whether they have been produced by the etching of a once perfect surface (Frank) or are "growth pits" (Tolansky). Artificial "trigons" can be produced by etching if the diamond is heated to 800°C in steam and chlorine, or to 500° in $NaNO_3$ melt. However, artificial "trigons" produced in this way are oriented with their sides parallel to the edges of the octahedral face whereas the natural "trigons" have the opposite orientation. This seems to favour the growth pit hypothesis, but Frank and Puttick[10] have been able to produce artificial "trigons" which have the same orientation as natural "trigons" by heating diamonds in fused kimberlite (blue clay) at 1450°C. However, Professor Tolansky (who has made a special study of "trigons" with his superb interferometric techniques) claims that all Frank and Puttick have done is to etch small pits around the boundary edges of a typical growth "trigon" not visible to them on the untreated surface. There I must leave this matter after drawing your attention to the very beautiful surface patterns which these "trigons" produce (Plate I).

Growth spirals, which have been found in many crystals, have never been found on natural diamonds. Tolansky has found them on the cube faces of a few synthetic diamonds though never on octahedral or dodecahedral faces. This would indicate that the conditions under which diamonds can be grown can vary considerably, and we undoubtedly have a considerable way to go before we can synthesise large diamonds of gem stone quality.

It has been estimated that to get the pressures required to synthesise diamonds it would be necessary to go to a depth of at least 100 miles. The South African diamonds are found in the "blue ground" in kimberlite which occurs in pipes and dykes formed by volcanic eruption, which probably occurred about the end of the Cretaceous period. Since diamond dissolves and graphitises fairly easily in molten kimberlite, diamonds could have been crystallised from the fluid if the carbon-rich kimberlite was under the requisite pressure and temperature and the latter was suddenly lowered by the release of gases.

Trigons on the surface of a diamond from Tolansky, *Microstructures of diamond surfaces.*

PLATE I

There is still a great deal to be discovered about the genesis of natural diamonds. How did the nitrogen get into the majority of all diamonds? Why are there some diamonds (Type II) without nitrogen? Why are the "Premier overblues" always Type II b and what impurity causes these diamonds to be semi-conductors? What is the origin of the diamonds found in meteorites?[11] There is a controversy here on whether the diamonds existed in the meteorite before it fell or were produced on impact. In the former case the meteorite must have been part of a much larger body at one time. The evidence for the latter view seems stronger.[12]

I referred to Newton at the start of my lecture and thinking of the Boer children who picked up the diamonds in 1866, I cannot do better than remind you of Newton's best known saying:

> "I do not know what I may appear to the world, but to myself I seem to have been only like a boy playing on the sea-shore and diverting myself in now and then finding a smoother pebble (or a prettier shell) than ordinary whilst the great ocean of truth lay all undiscovered before me".

I should like to acknowledge the valuable help and criticism which I had from Dame Kathleen Lonsdale, F.R.S., Professor S. Tolansky, F.R.S. and Dr. J. H. Custers in the preparation of this lecture.

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SIR GORDON SUTHERLAND

EXHIBITS IN THE LIBRARY

(*a*) A selection of coloured natural diamonds and a diamond in matrix.
Models of the Cullinan Diamond.
Some synthetic diamonds.
Pictures and coloured slides illustrating the extraction of diamonds.
A selection of diamond cutting and polishing tools.
Moisson's original paper on the synthesis of diamond and other historical works on diamonds and gems.

These exhibits were lent by *De Beers Consolidated Mines, The Industrial Diamond Information Bureau and Mr. P. Green.*

(*b*) Models of "tetrahedral" apparatus used in the synthesis of diamond, lent by the *National Physical Laboratory*.

DREAMING

By The Right Hon. LORD ADRIAN OF CAMBRIDGE
O.M., M.A., M.D., F.R.C.P., F.R.S.
Master of Trinity College, Cambridge

Weekly Evening Meeting, Friday 17th February, 1961

W. E. Schall, B.Sc., F.Inst.P.
Treasurer and Vice-President, in the Chair

IT is a remarkable fact that we have to spend about a third of our lives asleep—and to a physiologist, as I am, it is a remarkable fact that we really don't know why. Of course we vary a good deal in the amount of sleep we have to take: we can remain awake for 36 hours on end and in periods of emergency we can keep fairly efficient on a good deal less than the normal allowance. But healthy adults leading moderately active lives take an average of between 7 and 8 hours' sleep out of the 24, whatever the circumstances, at the North Pole or in the Tropics and in winter and summer.

The standard 24-hour cycle of sleeping and waking is partly a matter of convenience, fitting into the cycle of daylight and darkness. We can shift it without much inconvenience, when we go on night duty or fly to America, and in suitable conditions it is possible to shorten the cycle to 21 hours or lengthen it to 27, and there must be a good many people whose work prevents them from keeping to a regular cycle at all. But the alternating periods of sleep and waking have to succeed one another in something not much greater than the 24-hour period. We cannot accustom ourselves to sleeping more or less continuously for a week so that we may be able to stay awake for a fortnight on end.

If we could do so we should need less food in the week when we slept and more in the fortnight after it, and there is one kind of sleep that can be regarded as a way of avoiding starvation. In a cold winter a warm-blooded mammal needs more food to keep it warm and there is usually less food to be had. Many birds solve the problem by migrating south but some warm-blooded animals do it by hibernating, sinking into a torpor which lasts all the winter. This makes very little demand on the stores of energy in the body because the body temperature is allowed to fall to a level only a little above that of its surroundings.

But hibernation is not so much a sleep as a peculiar way of coming to terms with the winter cold, and animals which hibernate for seven months in the year are not continuously awake for the other five. The hedgehog sleeps for more than 12 hours out of the 24 all through the summer; the dormouse is said to sleep for 18.

At all events in man it is certainly not the need to economise in food that makes us go to sleep. It seems much more reasonable to look for something happening in the body in the waking period, something which is essential to activity but cannot be allowed to go on indefinitely without a period of rest to restore the *status quo*. It might be the accumulation of some waste product which is formed in the working period faster than it can be removed, or the using up of a store of some particular hormone or enzyme which cannot be replaced fast enough, or some redistribution of fluids or ions which has to be corrected in a period of rest. So far, however, no one has found clear evidence of an increased waste product or a reduced hormone level or a disturbed fluid or salt balance, or of any other accompaniment of activity which is cumulative and has to be corrected by sleep. All that we can say is that if the waking state is prolonged for 48 hours or more, the subject finds it increasingly difficult to concentrate his attention, to focus his sense organs and to carry out skilled actions; he finds it harder and harder to prevent his eyes from closing and he cannot allow them to close without falling asleep.

Up to that point we are sufficiently conscious and critical to give what we should regard as a moderately accurate record of our experiences. As soon as we are asleep our own record stops. When we wake up we may remember our dreams and give a report of them, but they are seldom vivid enough for us to be sure whether all the details we give were really part of the dream or whether we added them later to make the story hang together. But even though we have slept deeply and wake with no memories of dreaming it has to be recognised that when we are asleep we may still be in touch with what is going on outside us, at least to the extent of being able to distinguish sounds and to react to them not merely on the basis of their loudness but on their meaning. Being asleep need not prevent us from deciding which sounds are important to us and which can be neglected.

For instance it is the general belief that the anxious mother with a baby to nurse may sleep through a good deal of noise but will wake at once when her baby cries. There is so much folk lore about sleep (and about babies) that this example is not very convincing. Some people would think that it is a particular variety of sound with a specially disturbing quality, whoever hears it. But the point has been demonstrated recently in some pretty experiments made in Oxford by Oswald, Taylor and Treisman. The subjects were kept awake for 36 hours and then before and during their sleep a long series of names was read out regularly by a tape recorder. Their own name was in the series and they had been instructed to close their hand when their own name was called, or one other selected but otherwise indifferent name. The results showed quite clearly that the subject's own name was a much more effective stimulus and that he would usually react to the other selected name as well. Some of them closed their hand without waking, some woke, and some only got as far as a movement of the arm, and in some electrical records made from the head showed activity in the brain though there was no actual movement.

This use of electrical records from the sleeper's brain deserves some comment, for it is a relatively new method in investigations on sleep and it has given valuable results in Kleitman's recent work on Dreaming.

There is now a well-established technique for recording the electroencephalogram as an aid to medical diagnosis. As far as the patient is concerned it involves only the fixing of metallic electrodes to the scalp, for the potential changes which take place in the brain are large enough to record when the electrodes are connected by flexible wires to a suitable oscillograph. The same technique can be used on a sleeping patient and it has been found that the record of the potential waves shows characteristic changes as sleep becomes deeper or lighter. In the past the depth of sleep has often been measured by the intensity of the sound needed to arouse the sleeper, or less directly by observing movements, rate of breathing, etc. The potential oscillations in the brain show corresponding changes and they have the advantage of making it unnecessary to interfere with the sleeper as well as of giving a permanent record which can be studied at leisure.

It does not need such precise methods to show that the depth of sleep undergoes a periodic variation throughout the night. For the first 1½ hours the subject will sleep deeply, breathing slowly and regularly and making no other movement, but his sleep will then grow lighter, he will turn over in bed, breathe less regularly and be easier to wake. This lighter stage is usually followed by another period of heavy sleep and on the average the cycle of heavy and light recurs three or four times every night.

The most extensive observations of these cycles has been made by Kleitman at the University of Chicago and he has found recently that in the lighter stages of sleep there are usually periods when there are few general movements, but instead rapid side side to side jerking movements of the eyeballs. They occur in clusters and go on for from 3 to as long as 50 minutes. This movement can be recorded electrically: there is usually a rise in the heart rate and the rate of breathing when the eyeball movement is taking place.

Since this suggested that the subjects were dreaming, they were roused and questioned during or just after the period of eye movement. They would almost always report a dream, but if they were woken out of a deeper stage of sleep which had lasted some time they would say that they had not dreamt.

There is nothing very remarkable in the finding that most of us are more inclined to dream when our sleep has become lighter, but the conclusion that the eye movements may be an indication of dreaming is quite new. They might well be the scanning movements which would accompany visual imagery in the dream, and they give a method of studying the length of the dream period which leads to some very interesting results.

Kleitman has found a good deal to support his conclusion that a subject is actually dreaming when the rapid eye movements are taking place. Part of the evidence must come from the report made by the subject when he is woken and so a good deal may depend on the way he wakes and the way the record is taken. But memories of a dream fade quickly and it is significant that the recall of the dream was best when the subject was woken during or immediately after a period of eye movement. He was much less certain about it if he was woken 5 minutes after the eye movement had stopped and if he was allowed to sleep for 10 minutes

after they had stopped and had slept deeply he would wake without any recollection of dreaming.

It is, of course, well known that external stimuli, the sound of a bell, or a flash of light, or a drop of water on the face often lead to some episode in the dream incorporating the sensation, but it is found that they do so only if they are applied when there is a dream in progress, or rather if the eye movements are going on and the electrical record shows that the sleep is light. In that case the external stimulus can be used to give information about the course of time in dreaming, or rather about the rate at which dreaming goes on.

Our dreams may or may not convey any sense of a lapse of time, but it has been found (by Dement and Wolpert, 1958) that where a stimulus of this sort was incorporated into a dream and the interval between it and the moment of waking was precisely timed, the amount of dream action between stimulus and awakening was proportional to the interval and did not vary far from the amount of real action which might have taken place in the same time. The same sort of conclusion is reached by waking subjects after rapid eye movement had been present for 2, 5, 10 minutes or more and comparing the report of the dream with the length of the period of dreaming—or rather of the eye movements which are supposed to indicate dreaming. Kleitman has found that a short period of eye movements implies a short dream, one with relatively few incidents in it, but a long period of eye movement gives a long dream with many incidents and changes of scene.

At first hearing this may seem a surprising result. The literature of dreaming has many accounts of dreams containing an extended series of events but seemingly produced in a very short period of sleep. But it may well be that the actual incidents in such a dream could all be reported in a few minutes. In fact Kleitman and Dement's findings about the length of the dream relate to the amount of incidents, action, change of scene, conversation and so on in the dream, not to the impression of the passage of time which the dreamer reports. And they seem probable enough, for it is certainly difficult to suppose that we can think faster, form more auditory or visual symbols—when we are asleep than when we are awake. The idea that one can have a

lengthy dream in a few seconds is a hallowed tradition, but it is no more likely than the other tradition that a drowning man recalls the whole of his past life before he goes under for the third time.

These objective signs of dreaming, the electrical records of muscular movement and of the potential waves from the cerebral cortex, can tell us very little about the content of the dream. There are occasional pointers—for instance Kleitman records one where the eye movements were up and down instead of from side to side and the dreamer reported seeing a shower of leaflets dropped down from a balloon. But the use of these records of eye movement to indicate dreaming has led recently to another interesting development which revives the suggestion that the dream may serve some useful purpose. Dement, a colleague of Kleitman, has tried the effect of cutting short dreaming episodes by waking the subject as soon as the eye movements begin. Eight young male subjects were found to spend about 20% of the night in dreaming when they were not disturbed. They then spent several nights when they were roused whenever the signs of dreaming began. On successive nights there was a considerable increase in the number of times that a dream began and was cut short. They became anxious and irritable in the daytime and as soon as they were allowed to sleep through the night without interruption the time occupied in dreaming rose to well above its initial level—as though they were making up for lost time.

One might reasonably expect that repeated waking would make the subject irritable next day, but the surprising thing is that the irritability did not occur when the subjects were roused an equal number of times during a period of deep sleep without the signs of dreaming—and that when this had been done for several nights running there was no increase in the dreaming period when they were allowed to sleep all night.

Dement is naturally rather cautious in drawing conclusions, but he suggests that a certain amount of dreaming each night is a necessity and that the pressure to dream builds up during the successive nights, causing both the increasing frequency of attempts to dream and the longer time spent in dreaming in the recovery period. Kleitman suggests as an alternative that dreaming may be a habit rather than a necessity, like smoking or eating

sweets. One could think of other possibilities, that it is the light sleep that is important and not the dreaming that occurs in it, and there is the Freudian view that dreaming is a safety valve for the unconscious; but I believe the experiments are still in progress and we must wait and see.

Obviously there is still a lot of uncertainty and a great deal more to be found out. From the physiological angle we need to know much more about the state of the brain and the activity of the nerve cells in it when we are asleep, and we are still without any clear understanding of why we need to sleep at all. If Dement's findings are generally confirmed there is the further problem of why we need to dream, and become so cross if our dreams are cut short. But already these new results point to one quite important conclusion, for we have Kleitman's evidence that we do not dream any faster when we are asleep than we can think when we are awake. It is what one might have guessed but it is satisfactory to have some proof of it.

THE DEVELOPMENT OF X-RAY ANALYSIS

By SIR LAWRENCE BRAGG, O.B.E., M.C., D.Sc., F.R.S.
Fullerian Professor of Chemistry; Director, Davy Faraday Research Laboratory

Weekly Evening Meeting, Friday 3rd March, 1961

W. E. Schall, B.Sc., F.Inst.P.
Treasurer and Vice-President, in the Chair

It is now nearly 50 years since von Laue discovered X-ray diffraction by crystals, and I propose to survey the stages by which the science of X-ray analysis have developed since that time. It has played such an important part in all researches into the structure of matter because, for the first time, it afforded the scientist a way of discovering the arrangement of atoms. X-ray wavelengths are shorter than the distances which separate the atoms, and the strength of a scattered or diffracted X-ray beam depends on the interplay of the wavelets coming from the individual atoms. Hence by studying the strength of the diffracted beam one can draw conclusions about the atomic pattern.

X-ray analysis is generally associated with crystal structures, but this is a matter of convenience. It is easier to interpret the scattering by each element of atomic pattern when their elements are arranged in a regular way, as they are in a crystal, but the object of the study is the atomic arrangement and the crystal is merely a means to an end. The new method of analysis opened up quite a new field, one of the highest importance to many branches of science. Plans became available showing the precise positions which the atoms take up under the influence of the forces between them, thus casting new light both on these forces and on the consequent large-scale properties of the bodies. Not only physics but also chemistry, mineralogy, metallurgy and now biochemistry have been profoundly influenced by the new knowledge.

It is interesting to trace the converging lines of thought and experience which initiate a new science and determine its rate of development. In the first place the time must be ripe; there must have been a series of recent advances in knowledge which potentially enable the new advance to be made. At the same

time, it is often by chance that these advances, made in several branches of science, are brought together and that their joint significance is realised. The situation may be compared to that of a supersaturated solution, which requires a nucleus on which crystallisation can begin. It was a favourite saying of my father's that, after a year's research, one realises it could have been done in a week. This saying must of course not be interpreted literally; there are many cases when data must be laboriously collected before one can arrive at an answer. He was, I think, referring to the fumblings of thought, the exploration of alleys which turned out to be blind, the lack of knowledge of some vital piece of information which was there for the asking if one only had known where to ask. All of us must have said to ourselves at one time or another in our research, "How blind I have been".

X-ray analysis is particularly a subject where a review of these factors is interesting, because it has developed within living memory and it is so well integrated. It is a branch of research with a discipline of its own, and one with features rather unlike those of any other science. It depends on no abstruse principles and does not require apparatus of any great elaboration. Success in pursuing it demands that particular turn of mind which would lead to the successful cracking of a difficult cipher. Those who pursue it are a closely united band who talk the same language and keep closely in touch with each other. At the same time it is a typical border-line science.

It is convenient to divide the story into certain periods and I propose to take each in turn, discuss the important advances during the period, and sum up the stage which the subject had reached at its end. Our first period, starting with von Laue's discovery, may be taken to end in 1920. It includes the first world war, a time when little research was possible, but even during the war there were some outstanding contributions, and at the end of it a foundation had been laid on which most subsequent work has been based. The next decade, 1920–1930, witnessed X-ray analysis taking shape as a quantitative science, with the result that it could be extended to more complex structures. 1930–1940 saw a new way of analysing the results as a standard method of attack. Each diffracted beam was regarded as due, not to the combination of waves from discrete atoms, but to a

harmonic component of the structure as a whole. Just as the complex sound wave coming from a musical instrument represents the combination of a fundamental and overtones, so the variation of density in the crystal pattern can be thought of as due to the combination of harmonics in the form of density waves in all three dimensions. It was my father who first pointed out in 1915 that there is a one to one correspondence between the density waves and the diffracted beams. The strength of each diffracted beam is a measure of the amplitude of the corresponding density wave in the pattern. By adding these waves in a three-dimensional "Fourier" series, one can obtain a density map of atomic pattern.

The second world war interrupted research more completely than the first. Science played a far larger part in it, and most scientists had to abandon research, so in a general survey of this kind we may consider it as a blank period and take our next decade to be 1945–1955. To me this decade appears as the flowering of the trial and error methods of X-ray analysis, which except in a few favourable cases has been characteristic of X-ray work from the start. The amplitude of the waves scattered by the unit of crystal pattern in the direction of each diffracted beam can be deduced directly from its intensity, and this tells us the amplitude of the corresponding Fourier component of density in the crystal. This information, however, is insufficient by itself to form a map of the crystal pattern; one also needs to know the phase of each harmonic component with reference to some convenient origin in the unit cell. The intensities alone do not tell us what these phases are, and some way must be found of arriving at a set of phases which yield an answer compatible with the physical nature of the atomic pattern. This phase difficulty was accepted as a necessary concomitant of X-ray analysis, and the outstanding researches were characterised by brilliant tours de force in which success was attained in spite of it.

Finally, the decade in which we are now living, from 1955 onwards, is one in which a great stride forward has been made. The structures which are tackled are so complex that no trial and error methods could possibly succeed. X-ray analysis has assumed a new aspect in which the phase difficulty must be and

has been defeated and since the new methods are direct methods, machines can be our slaves to handle vast masses of data because they can be given clear and precise orders. We may perhaps liken these stages to geological eras, and say the primary extended until 1920, the early and late secondary from 1920 to 1940, the tertiary up to 1955, and that we are now living in a quaternary age of X-ray analysis. Any such divisions are of course arbitrary, and only justified as a skeleton plan on which to base our review. I intend to use as illustrations outstanding contributions which are landmarks, but it will be realised that it is impossible to refer to all such events and if the work of a particular school is reviewed in some detail, it is because it affords a measure of the advance of the subject, not because it was the only work of its kind at the time.

The original discovery of X-ray diffraction might very nearly never have been made but for a fortunate chance. Von Laue in 1912 had the idea that the regular arrangement of atoms in a crystal might diffract X-rays, in much the same way that the regularly spaced lines of a grating diffract visible light. He suggested trying the experiment to Friedrich, a young research student at Munich. Friedrich and his assistant, Knipping, arranged that a beam of X-rays should fall on a crystal of copper sulphate and placed a photographic plate in a position to record any spectra thrown back by the crystal. No effect was observed and the attempt might easily have been given up, had not Knipping, as rather a shot in the dark, tried an arrangement by which the plate was placed on the far side of the crystal from the X-ray beam. A pattern of diffracted spots was then observed surrounding the place where the direct beam from the bulb fell on the plate. In general the diffracted beams are only strong when the angle through which the rays are diffracted is not too great. This first photograph of Friedrich and Knipping and its interpretation by von Laue started the science of X-ray crystallography.

My father was intensely interested in von Laue's results because at the time he upheld the theory that X-rays were a type of corpuscular radiation, the "neutral pair" theory. This theory was not so strange as might appear. He had been led to it by deducing what we now know to be true, that when X-rays ionize

a gas the energy is handed over to individual electrons here and there in the gas, as if they had been struck by a projectile. It must be remembered that this was before the days of the Wilson cloud chamber, and before the dual wave and particle nature of radiation had become a familiar idea. We had many discussions about the results in the summer holiday of 1912, and when I went back to Cambridge as a research student in the autumn I pored over von Laue's pictures. Here again chance played a part. It must be difficult nowadays to imagine how utterly ignorant physicists were at that time of the geometry of three-dimensional patterns. I happened to know about it, because Pope and Barlow had proposed a valency-volume theory of crystal structure which although incorrect was highly suggestive, and a member of a small scientific society to which I belonged had given a paper on this theory. It brought home to me the fact that atoms in space lattices were arranged in planes! J. J. Thomson had been talking about the pulse theory of X-rays and, to cut a long story short, I explained von Laue's results as being due to the reflexion of a band of "white" radiation by the planes of the crystal. I published a note in *Nature* showing the reflexion of X-rays by a mica sheet at a series of angles. Further I showed that the spots von Laue had obtained with zincblende were characteristic of a face-centred cubic crystal, having heard about such a lattice by studying Pope and Barlow's papers. Pope was intensely interested and encouraged me to try Laue photographs of NaCl and KCl, in which he also believed the atoms to be in the close packing of a face-centred cubic lattice. These photographs established the structure of the sodium chloride group of crystals (NaCl, KCl, KBr, Kl).

This tentative start of crystal analysis, however, was soon completely superseded by my father's development of the X-ray spectrometer at Leeds, and I think one can trace the reason why crystal analysis was more rapidly developed in this country although it had started in Germany. My father was supreme at handling X-ray tubes and ionization chambers. You must find it hard to realise in these days what brutes X-ray tubes then were. One could not pass more than a milliampere through them for any length of time or the anticathodes got too hot. The discharge drove the gas into the walls; one then held a match under a little

palladium tube which allowed some gas to diffuse through and so softened the tube. The measurement of ionization with a Wilson gold-leaf electroscope was quite an art too, and my father had thoroughly mastered all the techniques in his researches. The great strategical importance of the ionization spectrometer was that it enabled reflexions to be studied one by one. The discovery of three characteristic lines *A*, *B*, and *C* of platinum was announced in April 1913 and my establishment of the rocksalt structure enabled wavelengths to be assigned to these lines. It is not generally realised, I think, to what an extent my father founded the science of X-ray spectroscopy in 1913. He had tubes made with anticathodes of platinum, osmium, and iridium, of palladium and rhodium, and of copper and nickel. He found the wave lengths of their characteristic lines, and by measuring absorption coefficients he identified them as Barkla's *L* radiation in the platinum group of elements and *K* radiation in the other groups. Whiddington had established the energies of the cathode rays required to excite Barkla's *K* and *L* radiations in atoms of different atomic weight, so my father was able to link wavelength with energy and show that the relation was in agreement with Planck's quantum law. He found the *K* line pattern and the *L* line pattern to be similar for successive elements, and showed that the frequencies increased roughly as the square of the atomic weight (June 1913). He identified the absorption edges, as they are now termed. The link with Moseley's work is interesting. Moseley and Darwin had surveyed the spectrum emitted by a platinum anticathode, but missed the characteristic lines owing to setting their slits too fine. My father's success encouraged Moseley to review the lines for a continuous series of elements from aluminium to gold. Bohr was then at Manchester, and Moseley's triumph lay in explaining the succession of frequencies by Bohr's theory of spectra and so identifying the atomic number as the number of positive electronic charges on the nucleus (April 1914), in accord with the view put forward by van de Broek in the previous year that the charge carried by the nucleus is in all cases an integral multiple of the charge on the hydrogen nucleus.

I had the heaven-sent opportunity at this stage of joining in the work with the ionization spectrometer. My father was more interested in X-rays than in crystal structure, and I was able to

use the spectrometer for investigating the latter. We published the diamond structure together, and I established the structures of ZnS, FeS_2, $NaNO_3$, and $CaCO_3$. The structure of pyrites, FeS_2, provided the greatest thrill. It seemed impossible to explain its queer succession of spectra until I discovered, going through Barlow's geometrical assemblages, that it was possible for a cubic crystal to have non-intersecting trigonal axes. The moment of realisation that this explained the iron pyrites results was an occasion I well remember. I tried to explain it to an aunt who happened to be in the room, with indifferent success.

I will select three further papers which appeared during this period as standing out from the rest. The first was a paper by my father on "The Intensity of Reflections of X-rays by Crystals". Crystals, though they often appear so perfect, are practically always composed of small blocks slightly tilted with respect to each other due to accidents of growth; this feature is called their "mosaic structure". It is therefore not sufficient to measure the reflection at one angle only, because at this angle only some of the blocks will be in just the right orientation to reflect. My father showed that one could get a true measure of the reflection by turning the crystal at a uniform rate through the mean reflecting position and adding up the results, so that each small block had its opportunity to add to the total which represented the true "integrated" reflection. This was the start of accurate measurement and diffraction.

Then in 1914 C. G. Darwin contributed two remarkable theoretical papers on the intensity of X-ray reflection, showing how strong it should be in comparison to the incident X-ray beam. These papers, appearing at the very start of the new science, are the foundation of all interpretation of X-ray diffraction.

The third contribution appeared in my father's Bakerian Lecture in 1915; a quotation from it will show its significance:

> "Let us imagine then that the periodic variation of density (in the crystal) has been analysed into a series of periodic terms. The coefficient of any term will be proportional to the intensity of the reflection to which it corresponds".

This paper was the foundation of what we now call "Fourier" analysis, and my father used it to get some idea of the distribution of scattering matter in the atoms.

Finally, a whole new range of crystallised substances became available through what is called the powder method, developed in 1916 by Debye and Scherrer in Switzerland and independently a year later by Hull in America. The X-rays are allowed to fall on a mass of minute crystals orientated in all directions. Instead of turning a single crystal round and measuring the diffracted beams individually, one relied on the fact that in the mass of powder there would always be some crystals which happened to be orientated the right way for each of the reflections, so that a photographic plate could register all the different kinds of diffraction simultaneously. Previously in order to get sufficient intensity, we used large single crystals in the ionization spectrometer. We asked our colleagues to provide specimens an inch or two across if possible, though we had to be less ambitious in the case of diamonds which fortunately had a low absorption coefficient and gave strong reflections. The powder method opened the way to examining microcrystalline material.

The next decade, 1920–30, witnessed a great advance in the application of X-ray analysis to more complicated crystals, particularly inorganic crystals. A rough measure of the complexity of a crystal is the number of "parameters" which have to be fixed, to define its structure. In the very simple crystals, the atoms have to be exactly in certain places in order to obey the symmetry requirements. For instance, in rock salt the sodium and chlorate atoms are exactly at the corners of cubes. Such a crystal has no parameters. It may be the case, however, that an atom must lie on a symmetry axis, but as far as the symmetry goes it may be anywhere along that axis. Its position is then defined by one parameter, the distance along the axis from some chosen origin, and this parameter must be deduced from the way the crystal diffracts. It may lie on a symmetry plane, when it has two parameters, or it may be in a general position when it has three. The first crystals to be worked out had at most one or two parameters. Now we began to be bolder and tackled crystals with ten or twenty. I remember well that at the time our colleagues thought we were being far too bold and that the results were extremely dubious, but actually they were all right. Three things helped and made the advance possible. In the first place, we were beginning to get some idea of the sizes of the atoms,

and this helped us to see how they must be packed in the crystal structure without overlapping. I first put this idea of packing together in 1920 and my sizes were corrected to a more reasonable set by Wasastjerna in 1923. Then general methods were developed by Niggli, Wyckoff, Astbury and Yardley for a direct determination of the symmetry elements of a crystal, the set of symmetry centres, axes of symmetry, and planes of symmetry in the pattern of the structure. This provided a kind of framework of reference in which we had to place our atoms according to the symmetry laws.

The main advance, however, was made by measuring the reflections accurately, comparing them with the incident beam, and using Darwin's theory of reflection in interpreting them. We chose the silicates to try out our new ideas at Manchester, not originally because they were important minerals, but because they were good material and we could borrow specimens from our friends in museums. The result of the research however was to put silicate chemistry on quite a different footing. It was a great thrill to see that the silicate minerals, which are the most important constituents of the earth's crust, fall into a simple scheme when their structures were interpreted by X-rays.

In this decade also my father broke new ground by measuring the first organic crystals, naphthalene and anthracene. The chemists had shown that naphthalene consists of two benzene rings joined together and anthracene of three such rings. My father measured the sizes of the cells into which these molecules fit in the crystals of naphthalene and anthracene, and by using the measurement of the distance between carbon atoms found in the diamond he showed that two rings just fitted neatly into the cell of naphthalene and three rings into that of anthracene.

The research school which he had gathered together at the Royal Institution went on to examine other organic compounds like the long chain hydrocarbons. In 1929 Mrs. Lonsdale made a complete determination of the structure of hexamethybenzene. This contribution from the Royal Institution was the start of the X-ray examination of organic crystals which has since grown into a very great field of research.

Towards the end of this period a new way of tackling crystal structures assumed importance, which was a development of my

father's original ideas on Fourier series mentioned earlier in this talk. This new way consisted in measuring all the X-ray reflections from planes parallel to a mean direction in the crystal or "zone". Their intensity is then interpreted as harmonic waves, which when added together gave a projection of a crystal structure on a plane perpendicular to the zone. The positions of the atoms were then seen as points of high intensity, easily represented by drawing contours of equal density like the contour lines of a map, when the atoms showed up as "hills" with contour lines running round them. This method very soon assumed a principal importance in crystallography. It involved a change in technique. Previously we had determined the positions of the atoms by measuring a small number of reflections very accurately and finding atomic co-ordinates which explained them. For the new method one had to measure all the reflections around a zone up to a certain limit. It was no longer convenient to use the spectrometer which measured reflections one by one, and photographic methods, where large numbers of reflections were recorded on a plate, came into general use, together with accurate photometric measurements of spot intensity. The new method was a trial and error method like the old one. One could measure the amplitudes of the harmonics with the X-ray results, but one had to guess their phases.

At first only centro-symmetric projections were examined because in these the phase must be either plus or minus, that is, the harmonic must have either a crest or a trough at the centre of symmetry. Later, a far more difficult extension to projections without a centre of symmetry, where the phases may have any value, was successfully achieved. The general procedure was to get an idea of the crystal structure from packing and chemical considerations, to calculate some of the main phases, make a first rough projection, which indicated further details of the structure and gave further phases, repeating this process of refinement until a satisfactory solution was found.

The next period, 1930–40, saw these methods applied to a large number of problems of crystal chemistry, in particular to organic crystals. In general the findings of X-ray analysis confirmed the ideas of the organic chemists about stereo-chemistry. The new contribution was the accurate determination of bond

lengths and bond angles and their interpretation in terms of the nature of the chemical bond.

The same period witnessed a great development in the understanding of the structure of alloys. Alloys are microcrystalline, so perforce the method of attack had to be the powder method. Westgren started his study of alloys in 1921 and was the pioneer in this field. His foremost disciple in this country was A. J. Bradley, who had trained with him and came back to Manchester to develop his methods. Bradley developed the powder method with a virtuosity which has perhaps never been excelled since, if indeed it has been equalled. He analysed highly complex structures and explored the equilibrium diagrams of binary and ternary alloys. His work, and that of others in this field, enabled the theory of alloy structures to be developed by Hume-Rothery and Jones. It is not too much to say that the principles of metal chemistry began for the first time to emerge.

This brings the story up to the beginning of the Second World War, and it is convenient to resume it at the end of the War and take as our next decade the period 1945–55. *Acta Crystallographica* started publication in 1948, providing the X-ray crystallographers with a journal of their own. A glance through the numbers of the journal shows the general trend during the period under review. The majority of papers represent the exploitation of the methods which had been developed or at least foreshadowed before the War, applied to organic compounds, inorganic compounds, and alloys. Many improvements to the techniques were made, resulting in the successful solution of more complex compounds, in increasing accuracy of the determination of the atomic arrangement, in the detection of the position of hydrogen atoms, and in the measurement of individual thermal vibrations in which Dame Kathleen Lonsdale has made outstanding contributions. The volume of work is so great that it is hard to single out individual achievements without a feeling that injustice is being done by not mentioning many others. I shall have to give up any attempt to summarise what may be described as the vast extension of crystal chemistry by the use of the established methods of analysis, and confine myself to what I see to be the outstanding achievements which broke new ground.

In the organic field, most determinations of structure had hitherto confirmed the structural models of the organic chemist. Now a very difficult kind of "sound barrier" began to be passed, that of telling the organic chemist something he did not already know.

I may perhaps select the determination of the structure of cholesteryl iodide by Carlisle and Crowfoot (Mrs. Hodgkin) in 1945 as an early example of such an achievement. Bernal's studies of the sterols in 1932 had given some indications of their structure, and in the interval the chemical nature of the sterol structure had been largely established by the organic chemists. Carlisle and Crowfoot, starting with clues to the phases indicated by the heavy iodine atoms, were able to fix the positions of the thirty-three carbon atoms in the asymmetric molecule, and so establish the sterol skeleton with precision. Then there was the neck-and-neck race with the organic chemists for the solution of the structure of strychnine. In 1947 Bijvoet in Holland obtained a first rough Fourier synthesis of the sulphate on the 010 plane, getting signs by comparing sulphate and selenate. In 1948 he attained a precise projection, with which only one model was compatible, unaware that just previously Sir Robert Robinson had succeeded in making the right choice between alternative models on chemical grounds. These papers which established the structure of strychnine are models of brevity, which might well be studied by our more diffuse writers! The results were confirmed by Robertson and Beevers.

"The X-ray Crystallographic Investigation of the Structure of Penicillin" by Crowfoot, Bunn and colleagues, is famous. The investigation lasted over four years. Success was the result of team work in which the chemists and crystallographers made essential contributions. To quote from the paper, "Throughout the whole of the X-ray crystallographic investigation of Penicillin we have been working in a state of much greater ignorance of the chemical nature of the compounds we have had to study than is common in X-ray analysis". A large variety of X-ray techniques was employed to establish the structure both of the degradation products and of the complete molecule. The description of the research is in itself a small text-book on X-ray analysis.

The "sound barrier", to which I have likened the surpassing of the bounds of the organic chemists' knowledge, was broken through with a loud report in the case of Vitamin B_{12}. Crowfoot and colleagues published their completed analysis in 1957 after eight years' study. The asymmetric molecule has the composition $C_{63}H_{84}N_{14}O_{14}PCo$, and a vital role in the analysis was played by the heavy cobalt atom. In some respects the chemical evidence was definitely misleading, and the establishment of the structure was mainly due to X-ray analysis. We all recognise this analysis of B_{12} as an achievement which stands in a class of its own.

This brings me to the present era, and here the thrilling feature has been the successful application of X-ray analysis to biological structures. The model of the spiral structure of the polypeptide chain, the α-helix of Pauling, Branson and Corey was first deduced on theoretical grounds supported by a study of the form of amino-acids and simple polypeptides. It soon received confirmation from the treatment of X-ray diffraction by helical structures in general by Cochran, Crick and Vand, and its application to the diffraction pictures yielded by artificial polypeptides. This analysis was used by Crick and Watson to explain the excellent pictures of nucleic acid (*DNA*) which had been obtained by Wilkins, and in 1953 they published their structural scheme of the atomic arrangement in *DNA*, which has aroused such intense interest and has led to further researches in a number of laboratories which have confirmed its essential correctness. The transform of the helical structures, expressible in Bessel functions, also led Watson to deduce the helical nature of rod-like viruses. The diffraction pictures of the globular viruses led Caspar to suggest that they were a cluster of similar protein molecules in a symmetrically arranged point group. Further developments by Crick & Watson and by Franklin & Klug have given us a much deeper understanding of the general architecture of virus particles. X-ray analysis has also been applied to the structure of collagen, and of muscle. These cases present the novel feature that one is dealing with the transform of a symmetrical arrangement which is not part of a space lattice. Advance at the present time is so rapid that I must be content with a very brief reference to these important researches.

The first and the most recent structures analysed by X-rays. The structure of rocksalt on the right was analysed in 1913 and is on the same scale as the myoglobin molecule analysed by Kendrew and his co-workers in 1960.

PLATE I

The analysis of the proteins myoglobin and haemoglobin by Kendrew & Perutz is more in line with former work in that the goal is the complete determination of the atomic positions in molecules of definite structure arranged according to space-group symmetry. The new feature is the high complexity of the structures which have been successfully analyzed. The molecule of Vitamin B_{12} contains 181 atoms and its analysis represented a landmark in the solution of a complex molecule by what might be termed classical methods. Myoglobin of molecular weight 18000 contains some 2500 atoms, and haemoglobin is four times as large. It would have seemed almost impossible to tackle such structures only a few years ago. The model of the myoglobin molecule is something quite new in science, and poses fascinating problems of the interplay of forces in this vast array of atoms and the way in which they determine the chemical behaviour of the compound (Plate I).

One feature of the analysis is the complete absence of trial-and-error methods, and the directness of the approach with no assumptions about the kind of answer to be expected. Nature has, as it were, given us an unexpected break. On the one hand there is the formidable complexity of a molecule containing many thousands of atoms. On the other hand, as Perutz has shown, the very size of the molecule makes it possible to attach heavy atoms, often associated with quite large complexes, in various places without a disturbance of the arrangement of the molecules in the crystal. As it were, the molecule takes no more notice of such an insignificant attachment than a maharajah's elephant would of the gold star painted on its forehead. The attachment of such atoms in a sufficient number of places makes possible a completely direct determination of phases and so a solution of the structure. Taking advantage of the presence of a heavy atom, or of the possibility of isomorphously replacing one heavy atom by another, is of course a classical device, but this addition of heavy atoms in various places is new.

Next, because the approach is direct, clear instructions can be given to our slaves, the electronic computers, to work out the vast number of calculations required. The collection of data has also become automatic, and the main application of the investigator's judgement is to the interpretation of the data turned

out by the machines. I am not minimising the ingenuity which has led to the discovery of how to attach the heavy atoms chemically, to the design and accuracy of the recording apparatus, and to the programming of the data fed into the machines. The new feature is that the element of guesswork has gone, and been re-

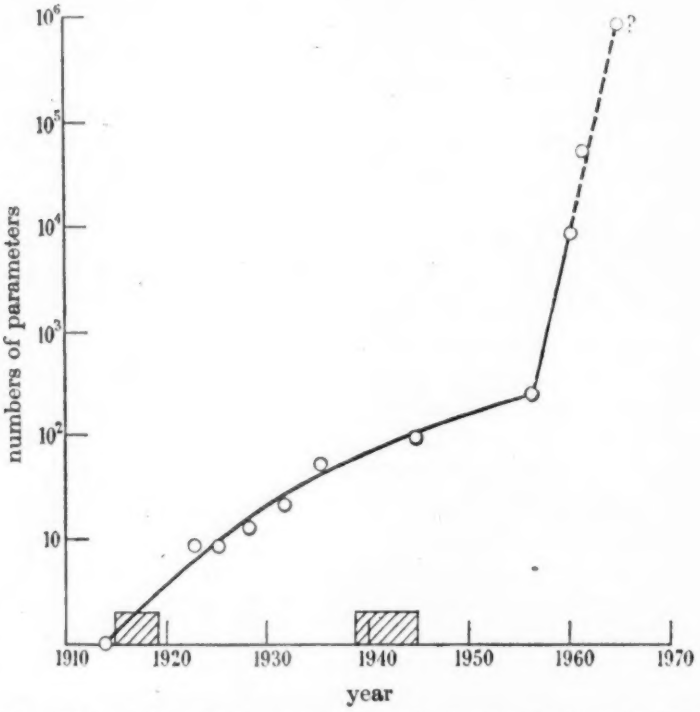


FIG. 1. The number of parameters in crystal structures analysed by X-ray diffraction is plotted logarithmically against the year. The hatched areas represent World Wars I and II.

placed by the handling of vast masses of measurements and calculations.

The nearly complete solution of the myoglobin structure by Kendrew, and the first outline of haemoglobin which will no doubt soon be also extended to a higher resolution, represent the result of some twenty years study of these two molecules. One cannot but admire the tenacity and faith which led to the final success after so many years with few encouraging results.

The other outstanding feature, to my mind, is the bridge which has been established between two ways of studying the structure of living matter. The electron microscope has been improved until now it is able to resolve bodies which are of the order of ten atoms across, so containing thousands of atoms. X-ray analysis has now reached a point at which it can place the atoms in molecules containing thousands of atoms. For the first time we shall be able to study the structure of matter right down to the atomic scale.

As I have worked for so long in this field, it will easily be understood how deep a gratification it is to me to witness the growing power of X-ray analysis and see how far it has progressed from the early days I remember so well.

I end this lecture with a graph (figure 1), in which the years are plotted horizontally, and the number of parameters in typical structure determinations are plotted vertically on a logarithmic scale. The curve starts with one parameter in 1913. It shows a sharp break at the exciting present time, and in tracing its course I have ventured with some confidence to assume that the haemoglobin structure will be completed next year. If we prolong the graph we conclude that we shall reach the million mark in 1965! This is not so improbable as it might seem; is there not hope that by that time we shall know the atomic architecture of a simple virus? How the million parameters will be listed in *Acta Crystallographica* is a problem I leave to the editors at the time. I have high hopes that I shall see the great day.

(A fuller version of this lecture was published in the *Proc. Roy. Soc. A*, **262**, 145–158, 1961.)

EXHIBITS IN THE LIBRARY

A display of (*a*) early crystal structure models and historic papers important in the development of X-ray analysis; (*b*) examples of early X-ray tubes and cameras; (*c*) models of Myoglobin, DNA, Vitamin B_{12} and Penicillin; arranged by *Sir Lawrence Bragg and the Davy Faraday Research Laboratory, Mrs. Dorothy Hodgkin and Dr. M. H. F. Wilkins.*

RELATIONS BETWEEN THE POLICE AND THE PUBLIC

By B. N. BEBBINGTON, O.B.E.
Chief Constable of Cambridge

Weekly Evening Meeting, Friday 21st April, 1961

Professor L. A. Jordan, C.B.E., D.Sc., F.R.I.C.
Vice-President, in the Chair

ON the 6th March, 1846 an American dwarf named Tom Thumb was exhibited at the Town Hall, Cambridge. For some reason there occurred, in the words of a contemporary writer, "a collision between some members of the University and the Police". The riots continued on the nights of the 7th and 9th March during which many were injured and much damage was done. Following the riots James Burdakin, a Fellow of Clare College, issued a pamphlet in which he complained that the Police had acted "rather as partisans with the town's people or rather town rabble, than as preservers of the public peace".

The history of Cambridge is peppered with incidents of this sort. The University 110 years ago was gravely suspicious that the new Police so recently acquired in Cambridge would be used by the Town Authorities as an instrument to enforce their supposed rights. Perhaps their dealings with the Corporation in the past was some justification for these fears. An enormous change has taken place in the past century. Differences of opinion between Town and Gown are no longer on the low level that the events of 1846 suggest and the Police are now accepted as impartial preservers of the peace.

Of course there are still differences of opinion between the University and the City Corporation, but such differences are mainly concerned with the replanning of the City to deal with the enormous increase of motor traffic. In Oxford it is the same. This shifting of the grounds for dispute is analogous to the change which has made itself evident in recent years in the background to the relationship between the Police and the public.

The coming of the motor car has undoubtedly put a strain on the harmonious partnership that should exist between the Police and the public, but it has also made the need for full co-operation between the citizen and the policeman greater than ever before.

Motor transport has provided the criminal with a quick and easy means of travel and it has caused much of the attention of the Police to be distracted from crime prevention and detection to the prevention of road accidents and the control of traffic.

The partnership between the Police and the public is unique, for nowhere else in the world is there a form of policing which depends so much for its efficiency on the support and active help of the citizen. It is a partnership of such importance that it would be indeed a tragedy if the citizen were to become a "sleeping" partner, or were to be so antagonised by the necessary enforcement of traffic and other legislation that he contracted out of the partnership. In any concern the partners ought to know each other's difficulties and peculiarities and there should exist between them a mutual trust. To some people the very word "Police" is an anathema. It conjures up in their minds the Gestapo, the tough Hollywood film cop, the shootings of Sharpeville. When an unarmed British "Bobbie" removes a pavement squatting "ban-the-bomb" marcher cries of "Fascist" come from some members of the crowd who do not know or do not wish to admit the vast difference there is between the democratic British policeman and the State-controlled tough of a totalitarian power.

The public have a very vague idea of the duties and responsibilities of the Police. They come into contact with the Service broadly in two ways. On the one hand they may be assisting the Police in the investigation of a crime or other incident, or seeking for some help and advice from the Police, or on the other hand they may themselves be the subject of Police investigation. Into whichever category their contact with the Police may fall their judgment of the Service as a whole rests largely upon the treatment they receive from the individual Police Officer dealing with them.

A great responsibility rests upon the shoulders of every Police Officer. This responsibility extends beyond the difficulties and dangers of law enforcement and embraces the need for him to be fair, courteous and impartial in all his dealings with the public. The public demand a high standard of efficiency and good behaviour from their policemen, and in spite of what is sometimes said I believe that the high standard is being maintained today.

The policeman on the beat is our best Public Relations Officer but we should not forget that policemen are human. They have their faults and failings, their difficulties and disappointments as do other human beings. The helmet contains no magic to change the grocer, or the bank clerk, or the engineer, or the undertaker's assistant into a supernatural being overnight. We do not want a policeman to become a cold machine-like minion of the State. We want him to remain human. When a young man joins the Police Service he receives a thorough training not only in the theoretical and practical aspects of Police duty, but also in the fundamental traditions of fairness in the service of the public. He learns that Police work has much more to it than catching a thief or summoning a motorist. Here I would like to lay a ghost. Many people seem to think that promotion in the Police depends upon the number of summonses that an Officer collects during his service. This is quite wrong. Before he qualifies for promotion to the rank of Sergeant or Inspector a policeman has to pass difficult examinations which thoroughly explore his technical and educational knowledge.

Sir Richard Mayne, one of the first Commissioners of the Metropolitan Police, issued advice to his Officers that when they were performing their various duties they should look on themselves as servants and guardians of the general public treating all law-abiding citizens, irrespective of their social position, with unfailing patience and courtesy. Today, 131 years after Sir Richard Mayne issued these instructions to his new Police, the recruit coming into the Service is instructed along the same lines. The policeman serves the public. He is not an employee of the State nor is he a Local Government servant. Each recruit, directly he has made his affirmation before a Justice of the Peace that he will "well and truly serve our Sovereign Lady the Queen in the office of Constable" takes upon himself an original responsibility in the performance of his Police functions. He must act only in accordance with the Laws of the country and if he makes a mistake—for example, if he makes an illegal arrest, he it is who may be sued for damages—not the State or the Police Authority or the Chief Officer of Police. This individual responsibility exercised by every Police Officer can be regarded as a considerable safeguard to the liberty of the subject. No high

Authority can give him orders to act in a manner contrary to the Law. He is not tied to the apron strings of any political party. He cannot be used to crush a minority movement, nor will he interfere with it if its objects and actions are legal. He cannot be used to "break" strikes or to intimidate an anti-Government agitator. His duty is to see that the Law as it applies in a particular situation, is obeyed by every member of the public no matter what that citizen's place in the social strata or what are his political or other affiliations.

Democracy in this country depends largely upon the right of the citizen to vote for the candidate of his choice at a Parliamentary or Local Government election. When you record your vote in a Parliamentary election you will probably go to a polling station in the local school or Church Hall. You may notice a uniformed policeman there and wonder what is the need for his presence. Elections are pretty tame affairs these days, but the sight of that Constable ought to remind you that 130 years ago the hustings were places of tumult, bribery and of gross intimidation. So used have we grown to democratic elections that we do not link the policeman's presence with our freedom to vote without fear for whatever candidate we have chosen.

It has often been said that a Constable is only a citizen paid to undertake tasks that could be performed by any other citizen. Every member of the public has certain Police powers which he has inherited from those citizen Police Officers, the tything man and the petty Constable. The new Police of 1829 came into being largely on account of the citizen's indifference to his duties of preserving peace and enforcing the Law, but Sir Robert Peel, in creating a new body for Law enforcement, was careful to make it civilian in character, and to leave with the citizen his existing Police responsibilities.

Relations between the Police and the public for a few years after 1829 were, as one can imagine, at a very low ebb. From the moment the first policemen stepped onto the London streets on the 29th September, 1829, they were attacked by the newspapers as being destroyers of liberty, a corps of spies and thoroughly un-English. After a few years public opinion changed. People began to see that these new policemen were really allowing them to enjoy a liberty they had not before experienced. Crime fell

and the Government were able to introduce social reforms which would have been impossible had there not been an efficient Police Service. Liberty at last began to mean something.

Today the citizen still has many of the powers and responsibilities that he had before the creation of the new Police in 1829. Unfortunately so few people know even what the Common Law demands of them in the way of Police duties. Few realise that under the Common Law, if any person sees a felony being committed (and a felony may be such a small offence as stealing a bottle of milk from a door step) they *must* arrest the felon. If they do not do so they themselves can be prosecuted and may be fined or imprisoned. Similarly if a Police Officer calls on a member of the public to help him, then if he is physically capable of rendering assistance that citizen *must* do so.

In these days people are very ready to talk about their "rights" as citizens and through the medium of the Press, radio and television they are learning more and more about these "rights". This is a good thing providing that the instruction given is correct and that an equal amount of instruction is given to the public regarding their responsibilities as citizens.

It has been said that a country gets the Police it deserves. There cannot be many people in this country who would like to see the Police armed, but if the public want to maintain an efficient and democratic Police it must actively support and co-operate with them. The alternative is more policemen and more powers for the policeman.

Whilst insisting on this high degree of co-operation from the public, the Police realise that there are many causes of friction between themselves and the public which upset the relationship. It is difficult to guage accurately what percentage of the population can be relied upon to support the Police and to undertake their full responsibilities as citizens.

As the result of a social survey conducted in 1951 by Geoffrey Gorer on behalf of a Sunday newspaper, it was found that 75% of the public were enthusiastic in their appreciation of the Police. Criticisms from the remaining 25% were mainly levelled at individual Officers and not at the Service as a whole. I do not believe that the relationship between the Police and the public has deteriorated since Mr. Gorer made his survey. It is, however,

clear that the causes of conflict between us and our partners have not grown less in the intervening years.

In the last 10 years road traffic has doubled and the Police are being brought more and more into contact with the public in their capacity as drivers of motor vehicles. Sometimes these contacts are irritating to the motorist, who on finding himself faced with a possible summons for what he regards as a technical offence, is inclined to say, "Haven't the Police got something better to do?". The motorist complains of the activity of the Police in preventing and detecting traffic offences, whilst the pedestrian complains of their inactivity in failing to enforce speed limits and in allowing vehicles to park on the pavement.

There will be no change in the attitude of the road user to law enforcement until the public realise that road accidents and traffic congestion constitute one of the biggest social and economic problems facing this country today.

In addition to the traffic problem and its consequent ruffling of our relationship with the public we have misunderstandings created by some legislation which is out-of-date, unpopular or obscure in its meaning. The Licensing Laws, the Laws relating to Betting and Gaming and the Sunday Observance Laws, are just some of those that bring us into sharp conflict with the ideas and actions of particular sections of the public.

The collection of evidence to prove offences gives rise to criticism of Police methods. People talk glibly about "agents provocateurs" when policemen enter clubs to find evidence of offences. What is not clearly understood is that when a complaint of irregularities in the conduct of a club or other establishment of a similar nature is received by the Police they cannot straightaway storm into the place and hope to find evidence. The complaint may be a malicious one, so observation must be kept before a Magistrate can be expected to grant a warrant to raid the premises. If it is a drinking club you cannot expect the Officers keeping observation to draw attention to themselves by sipping milk or lemon juice. In a gambling club it would be no good the Officers asking for the draught board. One would think that this is pretty obvious and yet some people are naive enough to believe otherwise.

Confessions are very much in the news these days. There are

suggestions that in the sinister back rooms at Police Stations expert Detectives are busy brain-washing or beating up innocent people in order to make them confess to some crime. No one in the Police Service has studied the techniques that are said to be used in China and elsewhere to make people confess. It comes as a surprise to us to hear that if a Detective offers a suspect a cup of tea or a cigarette when he is being questioned about an offence, it can be alleged that he is adopting a course of procedure recommended by the experts on brain-washing. He is said to take on by this kindliness the role of the Father Confessor.

The Police are anxious not to obtain a false confession from an innocent person, for it is of no value whatsoever and is an embarrassment to them. What they want is the truth—the facts. Crimes, especially those of a serious nature, are very carefully and thoroughly investigated. If there are "black sheep" amongst the Police then these are diligently sought out by the senior Officers and are dealt with either in the Courts or at disciplinary enquiries. We have a tradition for fairness in the Police Service of which we are very proud, and we do not "white-wash" the actions of those few policemen who would tarnish its tradition.

It should not be forgotten that Police work involves dealing with some members of the public who are unscrupulous, tough, degenerate, and merciless. It is often from these people that complaints occur regarding Police activities.

The Police have a dual responsibility. Whilst preserving a sense of fairness in dealing with a tough criminal they must also remember that one of their principal duties is to protect the public.

If the whole Police story could be told the public would be astonished at the number of their members who place great reliance upon the advice and assistance of the man on the beat. The lonely, the desolate, and the unwanted also find much comfort and encouragement from a few words with a policeman. Frequently a discharged prisoner goes back to see the Officer who arrested him in the knowledge that he will receive help and advice. Perhaps this sort of work may not be truly Police duty, and yet it provides an humane and human service for the public to balance the difficulties that arise through our enforcement of the Law. The faith that many citizens have in the Police is of very

great importance, underlining as it does the truly democratic traditions of service and guardianship which mean so much in our relationship as partners with the public in preserving the Queen's peace. The Royal Commission emphasised this aspect of our partnership in their Interim Report published in November, 1960 when they said:—

> "We cannot stress too highly the value to the community both in material terms and in terms of human happiness of an efficient and contented Police Service, adequate to present day needs and enjoying the confidence and support of the public".

EXHIBITS IN THE LIBRARY

A display of models and photographs illustrating aspects of the work of the Police; a selection of historic crime exhibits; specimen Police examination papers; Cambridge City Police Letter Book 1850 and Occurrence Book 1837; arranged by *Mr. B. N. Bebbington, City of London Police and New Scotland Yard.*

P.C. Herbert Dixon, dressed in the Police uniform of 1829, was in the Library before and after the Discourse to discuss the exhibits with Members and their guests.

THE SOUTHERN MILKY WAY

By BART J. BOK, Ph. D.
Professor of Astronomy, Australian National University and Director, Mount Stromlo Observatory, Canberra

Weekly Evening Meeting, Friday 5th May, 1961

L. B. W. Jolley, M.A., M.I.E.E.
Vice-President, in the Chair

THE band of the Milky Way outlines the position of the central plane of the stellar system in which our sun and earth are located. The greatest diameter of the Milky Way System or Galaxy is approximately 100,000 light years. Our sun and earth occupy a position well away from the centre but in or very near to the central plane, at a distance of about 27,000 light years from the centre of the Galaxy. As is the case for many galaxies beyond ours, the cosmically younger stars and the clouds of interstellar gas and dust are arranged in a spiral pattern and the tracing of this pattern is one of the principal tasks of the present-day Milky Way astronomer.

The southern Milky Way exhibits greater variety than the northern half. It is in the south that we find the Great Sagittarius Star Cloud, which marks the direction toward the centre of our Galaxy (declination 30° south). The regions richest in young stars and in interstellar gas and dust are also in the southern heavens near declinations 60° south in the constellations of Carina, Centaurus and Crux—the Southern Cross. The two Magellanic Clouds—satellite galaxies accompanying our Galaxy —are even farther to the south. All of this means that the few large southern hemisphere observatories—in Australia, South Africa and Argentina—have very special responsibilities (and opportunities!) for the study of the Milky Way and the Magellanic Clouds.

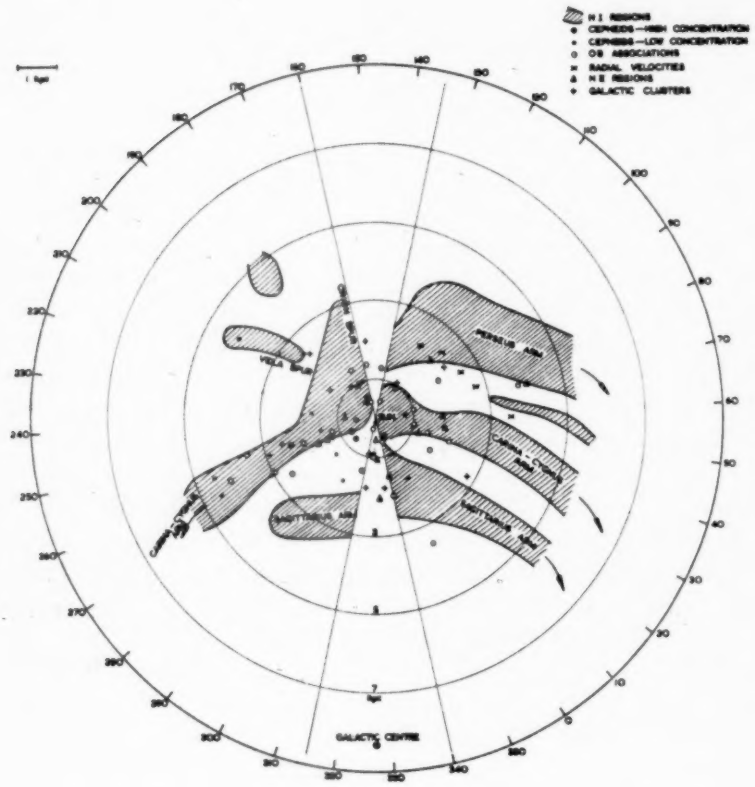
The first picture of the spiral structure of our Galaxy that proved worthy of serious consideration was suggested by W. W. Morgan of Yerkes Observatory and his two young associates, Donald Osterbrock and Stewart Sharpless[1], who traced three sections of spiral arms. The approach was a simple one: following a suggestion of Walter Baade's, Morgan plotted the projections on the central galactic plane of the positions of the blue-white

O and B stars, and of groupings of O and B stars. These stars and the luminous interstellar gas often associated with them had been found by Baade to outline most distinctly the spiral pattern in the Great Spiral in Andromeda. Baade and Morgan reasoned that the same varieties of stars and the interstellar gas of our own Milky Way System would show the spiral pattern of our Galaxy. The objects fell along three reasonably well-defined bands. The major one had in it OB groupings stretching from Cygnus, the Northern Cross, toward Orion and Monoceros in the opposite part of the Milky Way and it contains the sun at its inner edge. It was named the *Orion Arm*. It could be followed over a distance of approximately 10,000 light years. About 5,000 light years farther from the centre of our Galaxy than the Orion Arm, comes the outlying *Perseus Arm*, in which the famous Double Cluster, h and Chi Persei, is the principal feature. The third section of a spiral arm is at least 5,000 light years closer to the centre of our Galaxy than the Orion Arm and since it contains several groupings of O and B stars in the constellation of Sagittarius, it was called the *Sagittarius Arm*.

It was obvious from the start that Morgan's challenging and beautiful picture—which remains one of the classic diagrams of astronomy—suffered from some serious defects. It was drawn completely on the basis of observations made from northern hemisphere observatories and hence star groupings to the south of declination 30° south did not figure in it at all. This was unfortunate though perhaps unavoidable. It made the Morgan picture from the start incomplete, for the omission of the Carina feature made Morgan miss what is the most clearly and obviously marked spiral feature of our Galaxy.

There were two further defects of the Morgan picture that caused concern. Firstly, the sections of spiral arms drawn by Morgan were tilted by an angle of approximately 55° against the line from the sun to the centre of our Galaxy. This seemed surprising from the start, for, in most of the nearby spiral galaxies beyond our own, we observe more nearly circular spiral arms with tilts in the range of 75° to 85°. Secondly, parts of the Orion Arm—including the Orion Nebula and its associated grouping of O and B stars—are viewed from the sun in directions 15° to 20° to the south of the central band of the Milky Way. Since major

spiral arms are observed for at least one convolution in the normal spiral galaxies, it would be most unlikely to have them deviate very far from the central plane and it would indeed be



LOCAL SPIRAL STRUCTURE OF THE GALAXY

FIG. 1. The Local Spiral Structure of the Galaxy (Reprinted from *Observatory*, **79**, 61, 1959); "old" galactic longitudes are shown along the rim. The distance scale is in kiloparsecs (one kiloparsec equal 3258 light years).

surprising to find a major spiral feature point away from the central plane of our Galaxy by an angle of 15° to 20°.

The Mount Stromlo Observatory researches of the past four years have been devoted in part to the task of arriving at a more comprehensive and inclusive picture of the spiral structure of

The Radio Galaxy, Leiden Model, 1961. An "artist's impression" as drawn by G. Westerhout. The large circle with a central dot marks the position of the centre of our Galaxy and the smaller circle, also with a central dot, indicates the position of the sun and earth.

PLATE I

our Galaxy. Two years ago the author[2] suggested the diagram for the local spiral structure of our Galaxy shown in Fig. 1. The major local spiral arm becomes now the *Carina-Cygnus Arm* from which protrude two spurs, the Orion Spur and the Vela Spur. The major arms are more nearly circular in shape, centred upon the centre of our Galaxy and one cannot even say definitely from the sketchy optical evidence at hand that the arms are trailing—which most likely they are.

The picture that is shown in Fig. 1 was derived in part from optical researches, in part from radio work. We should note here that the optically observed radiations from the interstellar gas to which we made reference above are indicative of the presence of ionised hydrogen atoms, but no such radiation can be detected from clouds of neutral hydrogen. We have to turn to radio astronomy for further evidence. It was indeed most fortunate that at the time when we needed urgently information on the gases of interstellar space, Harold I. Ewen and Edward M. Purcell had just discovered the 21-cm radiation attributable to atomic neutral hydrogen. The Leiden group under J. H. Oort and H. C. van de Hulst was the first to follow up systematically —one might almost say majestically—the suggestion that from the study of the 21-cm profiles one might obtain important information on the spiral structure of the Galaxy, since neutral atomic hydrogen is presumably just as strongly concentrated in the spiral arms as is the ionised hydrogen. By combining the results from 21-cm studies of the northern Milky Way (and the accessible part of the southern Milky Way) from Leiden with those obtained at the Radiophysics Laboratory in Sydney by F. J. Kerr and associates, the whole of the Milky Way is covered 21-cm wise and a fairly complete spiral picture emerges[3]. Radioastronomical researches of spiral structure suffer, however, from the disadvantage that the precise interpretation of the 21-cm profiles depends basically again on optical information. Each profile shows, for a region in the sky of small angular extent, a plot of intensity versus radio frequency, which is in effect a plot of intensity versus radial velocity of approach or recession. Since the astronomer is interested in knowing the distances corresponding to the intensity maxima in particular profiles (which presumably represent points where the line of sight intersects a

spiral arm or spur), he must have a model of the velocity distribution in the Galaxy, which permits him to establish a velocity-distance relationship along each line of sight. Some information on the law of variation of circular velocity with distance from the centre of the Galaxy can be obtained from the radio data themselves, but this must be supplemented, checked and calibrated by direct optical means. Hence in the end, we require for dependable results as much optically-derived information as can be obtained to supplement the radio data.

Quite recently the radio astronomers have found it necessary to introduce one further complicating factor in the law for the distribution of circular velocity in our Galaxy. Kerr had noticed that there was something radically wrong with the spiral structure picture that emerged from the 21-cm data. When the accepted distribution law of circular velocities of galactic rotation was used to interpret the Leiden observations, the resulting picture of hydrogen distribution for the northern half of the Milky Way showed there reasonably well marked sections of spiral arms, but the derived picture for the southern half was confused and indicated the presence of gaps in the hydrogen distribution. It is obviously intolerable to have a marked asymmetry in results between the northern and southern half of the Milky Way. Kerr has suggested that the best way to achieve respectable north-south symmetry is by assuming an expansion velocity of the order of 7 kilometers per second at the sun in which all of the interstellar gas supposedly takes part. When one makes this assumption, the northern and southern half of the Milky Way give concordant results—and it becomes possible to trace the spiral features derived from the radio data as reasonably continuous spiral arms. The Galaxy seems to be a spiral in which the arms are winding up, but the angles between the direction of an arm at any point in the Galaxy and the line from that point to the centre of the Galaxy are so nearly 90° that from optical data alone—within limited reach in distance from the sun—one can hardly expect to distinguish marked deviations from near-circular appearance.

The most recent diagram of the spiral structure of our Galaxy drawn by the Leiden group is shown in Plate I. The gap in the information for the sector 15° to either side of the line from the

sun to the galactic centre is caused by our inability to distinguish from radio data alone between near and distant features.

There are various ways, through radio astronomical techniques as well as by classical optical means, in which one may check on the validity of the hypothesis of near-circular spiral arms.

Firstly, observations of the radio continuum, notably those of B. Y. Mills[4] made from Australia at a wavelength of 3.5 meters, show the presence of fairly well-marked sudden steps in the radio brightness distribution of the Milky Way. Mills considers these steps as indicating directions where one observes tangentially from the sun along one of the spiral arms. The schematic picture of the spiral structure of our Galaxy derived by Mills from the observed tangential directions is consistent with an overall spiral pattern in which the Perseus Arm represents the Sagittarius Arm one convolution later—and in which the Carina-Cygnus Arm is a section of the other major spiral arm of our Galaxy. We might add here that R. Hanbury Brown and C. Hazard have recently analysed Mills' data on the basis of the synchrotron-emission process, which involves emission by relativistically accelerated electrons, and they have found the Mills' picture quite consistent with the tightly wound spiral arms that we advocate. Some recent unpublished work by D. S. Mathewson, J. R. Healey and J. M. Rome at the Radiophysics Laboratory show that radio continuum measurements at 20 centimeters wavelength confirm several of the spiral edges found by Mills, notably the edge of the Sagittarius Arm at the southern end.

Secondly, optical evidence about variations in surface brightness along the band of the Milky Way suggest a somewhat similar state of affairs. H. Elsasser and U. Haug[5] have measured by optical photoelectric techniques brightness variations in two colours along the entire band of the Milky Way. Again they find certain directions of marked step-wise increases in surface brightness, which they consider to be directions for which we look edge-wise along a spiral arm. Their observations are of course much more affected by interstellar obscuration than are the radio data (which are unaffected by cosmic dust at the common radio wavelengths), but on the whole they support a picture

in which our Galaxy has two tightly-wound spiral arms, which, at the distance of the sun, have angles of the order of 75° to 85° with the direction to the galactic centre.

In a way the most significant recent new result has been the suggestion of Kerr that the interstellar gas near the sun is rushing outward at the rate of 7 kilometers per second. This is no small speed on the cosmic scale. A speed of 1 kilometer per second corresponds to a distance travelled of approximately one light year in 300,000 years. A speed of 7 kilometers per second means that a gas cloud would travel the distance from the centre of our Galaxy to the sun in a little over 1,000 million years and the gas would have presumably started on its way from the central regions of our Galaxy less than one-tenth of the probable age of our Galaxy ago. (The age of the Galaxy is estimated to be at least 10,000 million years). It is not unlikely that the outward motion of the gas would gradually slow down. The rate suggested by Kerr is probably smaller than the initial rate of expansion for the gas as it emerged from the central regions. Evidence to support Kerr's suggestion has been forthcoming from the Leiden radio astronomers, who have found strong indications that at a distance of less than 10,000 light years from the centre of our Galaxy outward gas motions prevail of the order of 50 or more kilometers per second. Kerr has tentatively suggested that his result is not inconsistent with an outward motion that varies in inverse proportion to the square of the distance from the centre of our Galaxy.

It seems most unlikely that the stars—at least the older ones—will share in this general outward motion. We know little about the precise nature of the forces that control the outward motion of a gaseous ring, which, after leaving the central regions of our Galaxy, might change to an expanding gaseous spiral arm. But we do know—at least the older generation of astronomers does, trained as they are in the dynamics of stellar motions according to Lindblad and Oort—about the motion of a *star* partaking in circular galactic rotation and possessing an outward motion of 7 kilometers per second. Such a star will describe a rather small epicyclic ellipse relative to a reference point in circular motion around the centre of the Galaxy—and it will most definitely not move outward continually. Hence a star formed

from a gas cloud sharing at birth in the outward motion of 7 kilometers per second should possess epicyclic motion and the gaseous complex that mothered it—spiral arm or otherwise—will gradually leave it behind.

At the Nantucket Meeting of the American Astronomical Society (June, 1961), Andrew T. Young of Harvard reported on attempts to study by high-speed computer techniques the effects of expansional gas velocities "at birth" upon the motions of the stars.

It should be possible to check on the reality of the suggested expansion velocity of 7 kilometers per second by comparative studies of the motions of very young stars and of stars of slightly more advanced cosmic ages (up to 100 million years or so), and relate these to the observed average motions of the clouds of interstellar gas—all for objects relatively close to the sun. The coudé spectrographs of the southern hemisphere have as a major assignment the making of such critical observations relating gas and star motions. Some work by P. Pismis of Mexico points to the presence of differential motions between very young groups of stars and somewhat older ones, suggesting expansional velocities for the younger groups, but the extensive analyses by F. K. Edmondson do not show the expected effects.

Here we should mention that the picture of gaseous streaming suggested by Kerr is not meeting with support from all concerned. J. H. Oort, the leader of the Leiden group, favours an approach in which it is assumed that the interstellar gas is streaming in the galactic plane *inward* toward the centre of the Galaxy—probably along the spiral arms with speeds of the order of 4 kilometers per second at the sun. He views the spiral arms as magnetically constricted tubes, which, because of the relatively permanent nature of spiral features, are kept in line by magnetic forces, and which are not as subject to winding up and tidal disintegration as would be gravitationally controlled spiral arms. Only direct optical spectral observation can decide between the two hypotheses, Kerr's of outward expanding gas motions, Oort's of a general mild inward flow.

We show in Fig. 2 the evidence available to date for the spiral structure of our Galaxy as summarised recently at Mount Stromlo Observatory. The diagram combines the most recent

21-cm picture of spiral structure with the optical evidence and gives equal weight to southern and northern hemisphere results. In this diagram the structure in the central regions is

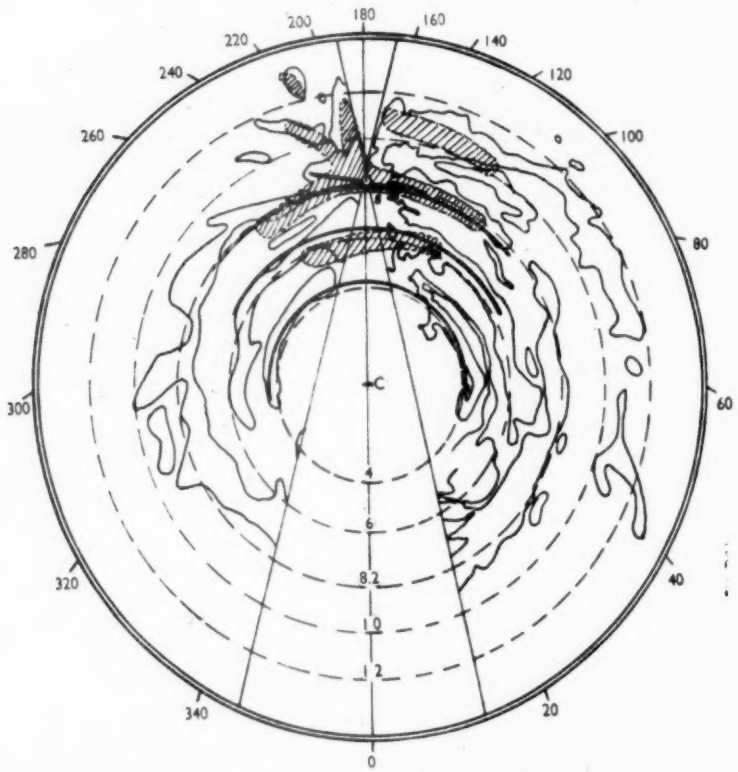
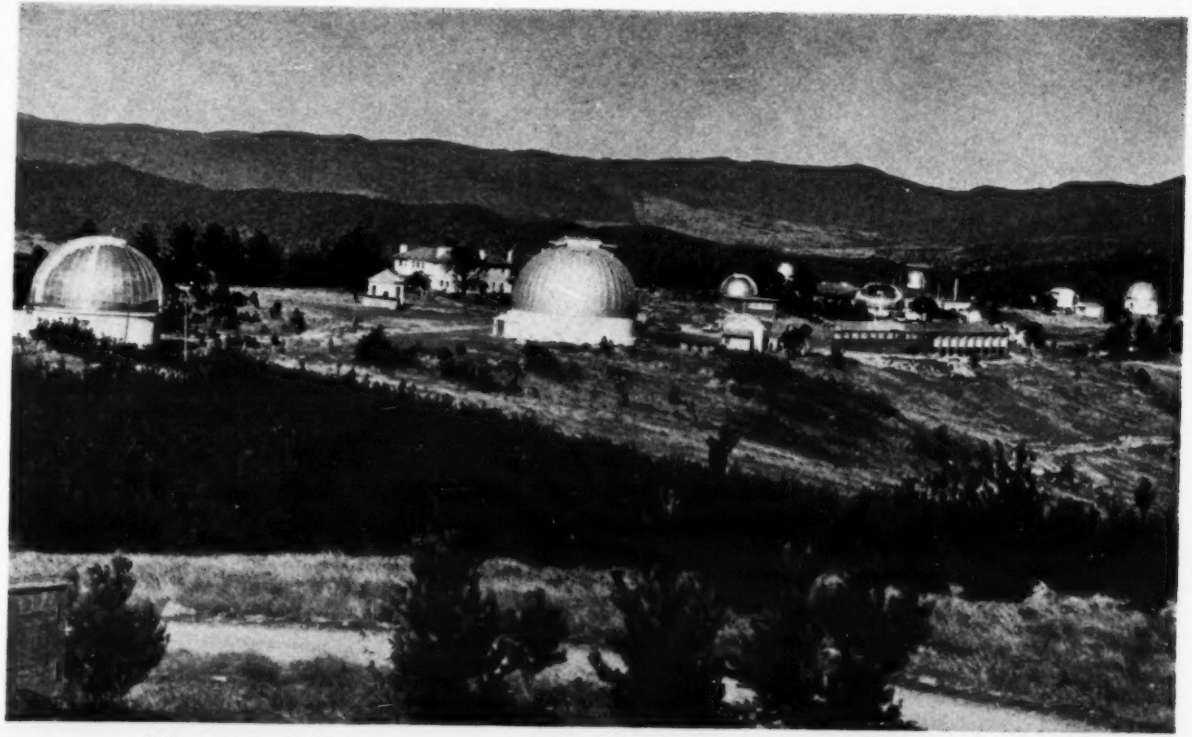


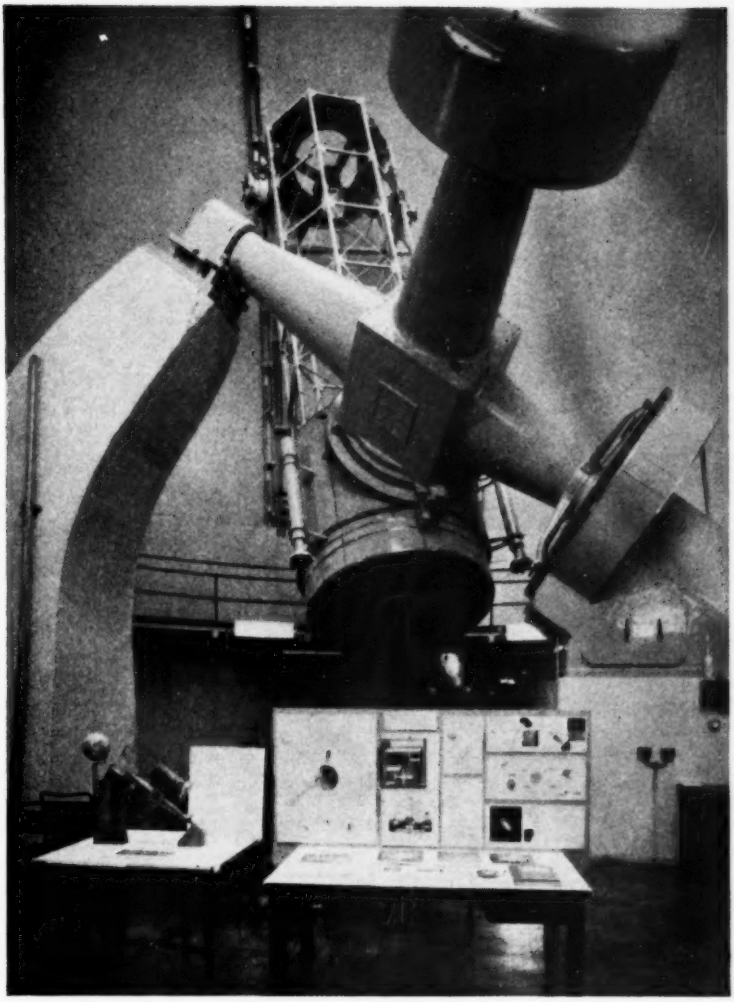
FIG. 2. The Spiral Structure of the Galaxy. (Stromlo Model). A comparison of models of spiral structure (1961) prepared by John B. Whiteoak. The contours are drawn from F. J. Kerr's representation of the Radio Galaxy—assuming general expansion for the gas of 7 kilometers per second. The shaded regions are copied from Fig. 1. The dark circular lines represent Whiteoak's interpretation of the optical data gathered by Elasasser and Haug [8]. The position of the sun is marked *S*, that of the centre of the Galaxy *C;* distance scale as Fig. 1. in kiloparsecs. "New" galactic longitudes are shown on the outer rim.

omitted but it is shown in Plate I. According to results obtained by the Leiden group and elsewhere (notably at the U.S. National Radio Astronomical Observatory at Green Bank), there is considerable gas in the central region of our Galaxy. This gas is contained in a disc relatively thin in directions perpendicular to the



Mount Stromlo Observatory of the Australian National University, Canberra, Australia.

PLATE II



The 74-inch Reflector: Mount Stromlo Observatory, Canberra, Australia

PLATE III

central plane of our Galaxy and it is expanding outward at a fast rate, ranging between 50 and 200 kilometers per second. There is strong evidence for an expanding gaseous ring—or arc, or possibly a section of a spiral arm—at 10,000 light years from the centre of our Galaxy. We thus find evidence for near-circular spiral arms all the way in the range of distance between 10,000 and close to 50,000 light years from the centre of our Galaxy.

It would be most unfair to give the reader the impression that everything is in perfect order with regard to the spiral structure of our Galaxy and that all workers in the field are fundamentally in agreement. W. Becker[6] has recently shown that many young galactic star clusters with well determined distances fall *between* the spiral arms drawn by the radio astronomers, rather than in them—as one might have hoped. In an extensive, as yet unpublished, study of the distribution in the galactic plane of the blue-white O and B stars, A. Beer of the Cambridge Observatories notes the existence of numerous O and B stars in the central regions of our Galaxy that fall decidedly outside the Sagittarius Arm—as this arm is generally drawn. On the other hand, H. L. Johnson and associates find from newly-derived distances for 70 galactic star clusters that the material gives fair agreement with the spiral features as drawn in Fig. 1 and 2.

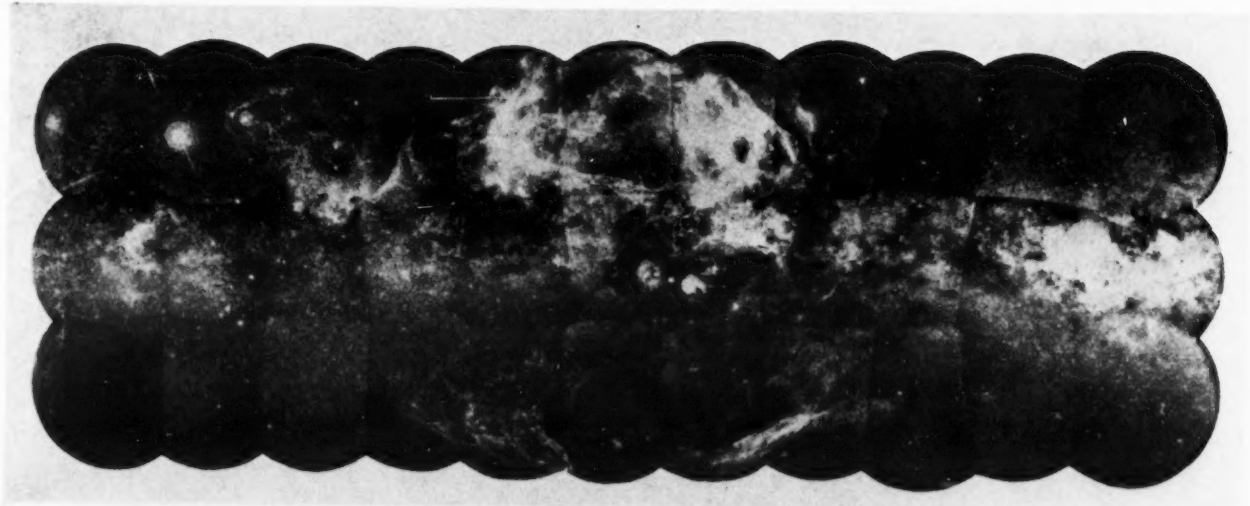
Polarisation studies in the optical range of wavelengths can assist materially in the disentanglement of the spiral pattern of our galaxy. The northern hemisphere work of J. S. Hall and W. A. Hiltner has shown that magnetic lines of force follow rather closely the course of the gaseous spiral arms. All stars viewed from the sun through a portion of a spiral arm more or less at right angles to the line of sight, show polarised light with the direction of preference aligned parallel to the direction of the spiral arm. On the other hand, stars viewed more or less along the direction of a spiral arm show randomly distributed directions of preference. From studies of the observed degree of polarisation and directions of preference in the light of distant stars we can hence decide whether for a given direction in the Milky Way we look either across one or more sections of a spiral arm or mainly along the spiral arm. The extensive existing optical polarisation data for stars observable from northern latitudes have already assisted us in the general checking of the spiral pattern within reach

of northern hemisphere observatories. Unfortunately little work in this area has been done from southern latitudes—at least since the preliminary survey by Elske van P. Smith, published more than five years ago. It is most urgent that southern Milky Way astronomers should undertake extensive polarisation studies.

It is a pity that interstellar absorption hinders so greatly our optical exploration of the spiral structure in the more distant parts of our Galaxy. Hence it is important that we should locate the most transparent directions along the band of the Milky Way, concentrating our efforts on the regions precisely on the central galactic equator. In our work at Mount Stromlo Observatory we have hunted for transparent regions where we might be able to observe tangentially, or nearly so, along the Carina end of the Carina-Cygnus Arm and it is a pleasure to report that a remarkably clear region is now known in just about the desired direction. It is Kapteyn Selected Area 193 near "old" galactic longitude 263° in Fig. 1 and "new" galactic longitude 296° in Fig. 2. The Stromlo results by Priscilla F. Bok, Jane M. Basinski and myself suggest for this direction a total visual obscuration of only about one stellar magnitude at a distance of 10,000 light years from the sun. Unpublished work by A. D. Thackeray, M. W. Feast and A. J. Wesselink at Radcliffe Observatory in S. Africa, and also by Dr. and Mrs. Th. Walraven at the S. African Leiden Station near Hartebeestpoortdam, confirm the Stromlo result. The Radcliffe observers find for this same section of the Milky Way only 20% greater total visual absorption at distances from the sun of the order of 15,000 light years. The preliminary Stromlo results indicate that the luminous O and B stars are distributed quite uniformly along the line of sight, thus confirming that in this direction we are looking tangentially along a spiral arm. In this section of the Milky Way the search for very distant blue-white O, B and A stars can be readily pressed and we should be able to obtain within a few years rather complete information about the distribution in depth of all relevant varieties of stars. In addition to the O and B stars, the search for and the study of variable stars should be continued for this section and important results can be expected from the work already under way for Kapteyn Selected Area 193 by Miss Margaret Olmsted in America and by Father Martin

The Great Nebula near Eta Carinae, 5000 light years from the sun. Photographed in Eastman 103a-E emulsion with the Uppsala Schmidt; 90 mins. exposure. Mount Stromlo Observatory, Canberra, Australia.

PLATE IV



From Atlas of H_2 Regions in the Southern Milky Way; Mount Stromlo Observatory, Canberra, Australia,

PLATE V

F. McCarthy of the Vatican Observatory. The study of all galactic clusters in the region is also a must and equally important will be the study of the gaseous nebulae in and near it and of the clouds of gas that produce the interstellar absorption lines in the spectra of distant stars.

I have purposely dwelt rather long on the work for Kapteyn Selected Area 193, for it indicates the direction of research along optical lines that must be favoured if we wish to obtain optical anchor points at great distances from the sun for use in studies of spiral structure by the radio astronomers. Already there are indications of a few more such relatively clear sections right on the galactic equator. We need them not only in the directions where we expect to look tangentially from the sun along a section of a spiral arm, but fully as much for directions in which we cross two or more sections of different spiral arms. The Radcliffe Observatory astronomers have already indicated that a most profitable direction may be "old" galactic longitude 295°, "new" galactic longitude 328°.

A word of caution is, however, in order. All observational evidence available to date shows that in the central and inner parts of spiral arms cosmic dust is almost always present. We must, therefore, proceed with caution in our analysis of very clear regions—for we may look in these directions at sections of the Milky Way already outside the truly central spiral features. In the end we can hardly hope to avoid inclusion of some rather obscured regions in our analysis, for the basic spiral features are still the places where cosmic gas, interstellar dust and very young stars are intermingled.

In the years to come special attention must be given to the study of the southern Milky Way, for it holds the key to the solution of the most important problems of Milky Way structure. The northern hemisphere astronomer looks, alas, toward the least interesting half of the Milky Way. If we wish to explore the important sector between southern declinations 40° and 65°, then we must go south to do the work.

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EXHIBITS IN THE LIBRARY

(*a*) The first showing of the Mount Stromlo Atlas of the Southern Milky Way by A. W. Rodgers, C. T. Campbell, J. B. Whiteoak, H. H. Bailey and V. O. Hunt. This Atlas represents the entire Southern Milky Way in 5 mosaics each prepared from 18 separate enlargements of plates in H-alpha light photographed with the f/1, 8-inch Schmidt Camera of Mount Stromlo Observatory. It covers a band 30° wide centered upon the equator of the Milky Way.

(*b*) 16 large-scale photographs to illustrate Mount Stromlo Observatory and its work, including several of the magnificent nebulae and clusters of the Southern Milky Way and the Magellanic Clouds.

LIBRARY CIRCLE MEETING

Thursday, 2nd March 1961

THE FUNCTION OF A UNIVERSITY PRESS

By A. K. PARKER, M.A., M.R.I.
Assistant Secretary, Cambridge University Press

James Lawrie, M.R.I., *in the Chair*

In any discussion of university presses it is necessary to distinguish between those such as Oxford and Cambridge which are both printing and publishing houses, and those on the American pattern which are solely publishers. The Oxford and Cambridge presses, and those of many European universities, were printing houses long before they became publishers and their early operations were based on patronage rather than on commercial considerations. Publishing is a mass production industry and requires communications with as large a body of potential book buyers as possible if it is to be economically self-supporting; before the railway age, few university towns were situated near enough to great cities or to ports for them to be able to distribute their books widely and cheaply.

The present operations of the Cambridge University Press arise directly out of the nature of Cambridge University. A first charge is, of course, the need to undertake printing required in the administration of the University; under this head comes the printing of statutes, of the *Reporter* and of examination papers (both for the University itself and for the boards appointed to examine in schools). This is jobbing printing and few publishing problems arise from it.

The publishing activities of the Cambridge University Press are related to the University's dual function as a place for research and for teaching. There is a duty to publish works of scholarship having a limited market, but which are perhaps of very great importance to specialist scholars and to posterity. But, of course, unless a press is subsidised by the university which owns it, as Cambridge is not, its ability to publish such works depends on its success in publishing others which are expected to show a profit. Here, a university press is in

direct competition with the commercial publishing house.

A university press does not, of course, have any monopoly of the publishing of textbooks used in the university which owns it or written by the university teaching officers. It is natural though that its ties with the teaching work going on on its doorstep are closer than those of any other publishing house, and a university press has a particular duty to make available textbooks needed in the university, for instance by keeping in print those that are in regular use, even if the demand is relatively small.

It is true to say that the whole of the Cambridge University Press's list is educational in the broad sense. Apart from works of scholarship and university textbooks, it also includes textbooks for schools—an aspect of publishing instituted in 1874 at the suggestion of the Cambridge Local Examinations Syndicate—and books of an educational character for the general reader. In the publication of books intended to interpret modern scientific advances to the non-specialist, the Cambridge University Press was something of a pioneer. Between the wars, it published many books by Sir James Jeans, Sir Arthur Eddington, Sir Charles Sherrington and other scientists with a gift for exposition. Some of these books were remarkably successful: in particular, Jeans' *Mysterious Universe* has sold about 400,000 copies in English and has been translated into more than fifteen other languages.

To the modern university, the possession of a press has become something of a status symbol, though unless the output is considerable, it is less costly and more efficient to have an arrangement with an established press, as Durham University does with Oxford, for example, than to start a new one from scratch and build up an entirely independent organisation. Liverpool, Manchester, the University of Wales, London (the Athlone Press) and Edinburgh have all established their own publishing organisations.

Conditions in the United States are different and there, owing to the extreme competitiveness of the market, the proliferation of book clubs, the absence of good bookshops on the British pattern and the high costs of printing, the university presses provide almost the only channel for the publication of scholarly books. Most American university presses are subsidised by their universities, either directly or by the provision of staff and free ac-

commodation on the campus etc. The products of the American university presses tend to be rather expensive by our standards and this militates against their world-wide sale.

Other countries too have their university presses and Toronto and Melbourne are notable examples in the English-speaking world.

To supervise the operation of its press, the University of Cambridge appoints a Syndicate of fourteen university teaching officers, who each serve for periods of seven years and are eligible for reappointment. The chairman is the Vice-Chancellor, but in practice he appoints a deputy to act for him, and the University Treasurer is also an *ex officio* member of the Syndicate.

The Syndicate, at its regular meetings and those of the committees appointed by it, controls the general policy of the Press, accepts or rejects manuscripts offered for publication and authorises major items of expenditure on plant or buildings.

The present annual output of the press consists of about 100 new books, perhaps 30 new editions and over 100 reprints, together with the regular production of some 40 learned journals, a few of which are owned by the Syndics and the remainder by learned societies. The total number of titles in print is around 4,000, a size of list which creates considerable distribution problems; Oxford University Press, with a still larger list, has even greater difficulties to overcome.

In the publishing of books on highly specialised topics, costs can only be reduced by ensuring that each book reaches the greatest possible proportion of potential readers. It is therefore the Syndics' policy to acquire world rights wherever possible and to market the book themselves, through their own branches in the U.S.A. and West Africa, and through agents elsewhere. As a result, about two books out of every three go for export and the Syndics' income from overseas sales is 57% of the total. The main foreign markets for books in English are the U.S.A., Canada, India, Australia, West Africa, Europe and Japan. There is also a substantial sale of translation rights—mainly into German, Italian and Spanish—and, nowadays, paperback rights.

The operations of the Cambridge University Press are characterised by a curious mixture of tradition and modern efficiency. On the one side one can quote the case of an author who took

44 years to prepare an edition of Cicero and point to the elderly and inconvenient buildings in which the printers are housed. On the other side, there is the recent development of a rotary press using flexible plastic plates which offers great prospects of reducing the cost of printing both long and short runs of books, and the plans for the new printing house which is now under construction.

The most significant feature in present day learned publishing —and particularly on the scientific side—is the increase in the size of editions. Whereas 1000 was quite a common printing number before the Second World War, 2500 is now about the minimum number which can be published at a competitive economic price. Fortunately this increase has been matched by an expansion of the academic market generally and by the growing appreciation outside academic circles that scientific results are of more than academic interest.

While this ability to market larger editions has kept down the cost of books to the purchaser, it has also increased the need for working capital. This is required to meet increases in costs brought about by inflation, to finance the printing and warehousing of larger editions and also, of course, to replace obsolescent plant and buildings. Profits are the only source of capital for an unsubsidised university press and recent Budgets have not made its accumulation any easier!

The other major trend is in the publishing of paperback editions. The Penguin-Pelican type paperback with its low price and printing numbers usually in the 30,000-100,000 range has been familiar for several years. The other type, selling at prices in the 7/6 to 21/- range and with printing numbers of the order of 10,000, is more of a newcomer to British bookshops. It has however been familiar in the U.S.A.—where it is called the "egghead" paperback—for several years but was not imported much into Great Britain. Now the trend has suddenly changed, and a large number of British publishers have followed the Cambridge University Press's lead early in 1960 to publish paperback editions of serious works of general interest in this price range.

A few years earlier, the Cambridge University Press had experimented by publishing a university textbook of physics in both cloth and paper covers, slightly weighting the price of the

cloth edition so as to be able to make the paper edition slightly cheaper than it otherwise would be. This experiment has proved an undoubted success and it is now the policy of the Syndics to publish "students' editions", as they are called, of new small textbooks and of reprints of textbooks wherever this is economically possible. This has helped to bring about a renewal of book buying on the part of undergraduates and has certainly benefitted both authors and publisher.

ROYAL INSTITUTION NOTES

OPEN DAY, 1st May, 1961

FOR the second year in succession an Open Day for Members and Subscribers was held at the Institution. On the occasion of the Annual Meeting, on 1st May 1961, aspects of the general work of the Institution were illustrated in the Ante-room and the Library rooms, and some of the research laboratories were open to visitors.

The exhibits illustrating the general work were grouped in the following sections.

(*A*) Historical apparatus, and manuscripts from the archives.

(*B*) Examples of experiments demonstrated at the lectures for schools.

(*C*) A selection of some of the newer apparatus specially acquired for practical teaching.

Section A

Selected letters were exhibited from the collection of manuscript correspondence between Faraday and Sir James Chance presented by Sir Hugh Chance, M.R.I. in 1960. These letters are concerned with Faraday's work for Trinity House in connection with lighthouses and show the interest which he took, especially during the later years of his life, in finding the most efficient means of lighting for lighthouses.

Also on show was a number of pieces of apparatus selected from the collection left by the late William Spottiswoode (1825–1883) and presented to the Institution in 1889 by Hugh Spottiswoode. Two heliostats, a Jordan sunshine recorder, a spectroscope with ten prisms, a spectrometer, a polarimeter, apparatus for showing diffraction, a Serrins arc lamp, and apparatus to illustrate the electromagnetic rotation of liquids.

Three early clock escapements were on view, as were examples of early electrical influence machines, the electrophorus, Nairne's travelling electrical apparatus, a cylinder machine, the Winter machine, the Wommelsdorfche kondensatamachine, and two Wimshurst machines. Some of these were operated to illustrate various effects of static electricity by means of ancillary apparatus.

Section B

Lectures for children have been a feature of the Institution's activities since 1826, when Michael Faraday was Resident Professor. The Christmas Lectures, "adapted for Juvenile Auditory", have been given each year since that date, with the exception of the four years during the last war, and have become famous for the excellence of their presentation and experimental demonstrations.

In 1954 a new series of lectures to young people was started. These lectures are given to children attending schools in London and the Home Counties. Ten different lectures are now given each year, each lecture being given four times. In this way some 20,000 children attend a lecture at the Institution each year.

On show, and actually demonstrated were a few of the many experiments conducted in the course of these lectures. These comprised:—demonstrations of fluorescence including electroluminescence, photoconductivity and semi-conductors, with working models illustrating the underlying principles; a series of experiments demonstrating electromagnetic induction effects, including three-phase generation, and the production of high voltage by induction coils and Tesla apparatus. An induction coil by Apps, reputed to be the largest ever made with a mechanical make and break, was shown in operation; apparatus for the generation and detection of 8mm. electromagnetic waves, presented to the Institution by *Marconi's Wireless Telegraph Co. Ltd.* By means of this apparatus, optical effects which are on too small a scale may be demonstrated to a lecture audience.

Section C

The importance of experiment and demonstration in the teaching of science has always been emphasized in the Institution. Even today there are many schools where facilities and equipment are inadequate for all the practical work which should be done, and indeed few schools are able to afford a complete collection of the apparatus now available for teaching.

The Institution has, therefore, helped by generous grants from the *Shell Petroleum Company* and *Imperial Chemical Industries Ltd.*, started to build up a collection of items which, for numerous reasons, it might be difficult for many schools to acquire. It is the aim that school teachers should be enabled to bring small groups

of their pupils to the Institution, and hold practical classes, using this equipment.

Some recent additions to the collection were demonstrated. They included apparatus for the measurement of the velocity of light; the field emission microscope and Van de Graaff generator; a fine beam cathode ray tube for the determination of the specific charge e/m of the electron; apparatus for the Frank-Hertz experiment on inelastic collisions of electrons with mercury atoms; the Wolff projection electroscope and accessories; and a set of component parts from which various electric motors and generators may be assembled.

Research

The occasion gave an opportunity for Members to make closer acquaintance with some of the research work reported to them in the *Annual Report of the Davy Faraday Laboratory Committee.* Apparatus used in the preparation and study of single crystals of metals was demonstrated and the results of recent work on the deformation of crystals of cadmium were shown. The linear diffractometer, designed and built in the Laboratory, for the automatic X-ray diffraction study of complex molecules was demonstrated in operation in the examination of sperm-whale myoglobin to a high degree of resolution and the investigation of the nature of the damage caused in such crystals by X-radiation. Models showing the interpretation of the latest results on sperm-whale myoglobin were on display, together with the results of closely related work on seal myoglobin. A new form of automatic diffractometer being developed in the Laboratory—a three circle goniometer with completely automatic setting of crystal and counter, incorporating many new features, was also demonstrated. Apparartus in which polarised X-rays are used for determining absorption corrections was on view.

Work proceeding on other substances, β-lactoglobulin, haemoglobin, lysozyme and complex mercury compounds was also illustrated, and X-ray tubes and a selection of cameras used in diffraction studies were shown. Finally, an exhibit was arranged to indicate the manner in which the large amount of data necessary for the study of complex molecules is handled and processed for interpretation with the aid of electronic computers.

MEMBERSHIP OF THE ROYAL INSTITUTION

Members of the Royal Institution support by their subscriptions an institution devoted to the prosecution of scientific research and to the diffusion of science and of useful knowledge. Its dual purpose of research and exposition is served by the scientific work carried on in its laboratories and by its Friday Evening Discourses and Lectures on science, literature and the arts.

MEMBERSHIP. Candidates for membership must be proposed from personal knowledge by four Members, who certify that the candidate is attached to science and desirous of admission into the Institution. It is not required of an intending Member that he or she shall have made any contribution to science. Persons normally resident overseas but residing in the United Kingdom for a limited period may be similarly admitted without payment of an admission fee.

SUBSCRIPTIONS. The Admission Fee payable on election is three guineas and the Annual Contribution is seven guineas; or the Life Composition in lieu of all payments is in accordance with a scale depending on age, with a maximum of one hundred and nineteen guineas.

PRIVILEGES. Members may attend the Friday Evening Discourses, and they may give two signed tickets for each Discourse to their friends; they may attend the special course of Christmas Lectures for Juveniles; they have the use of the Library.

ASSOCIATE SUBSCRIBERS. Persons between the ages of seventeen and twenty-seven are admitted, on the proposal of two Members, as Associate Subscribers, who may attend the Friday Evening Discourses and Afternoon Lectures, and have the use of the Library. They pay an annual subscription of two guineas.

CORPORATE SUBSCRIBERS. Any Company or Corporate body may be admitted as a Corporate Subscriber on the proposal of two Members. They receive (i) two tickets, each of which entitles any one director, officer or employee of that Corporate Subscriber to use the Library, (ii) two tickets for the Friday Evening Discourses, (iii) two tickets for the Afternoon Lectures and (iv) a copy of the *Proceedings*. They pay an Annual Subscription of 50 guineas or more.

LIBRARY SUBSCRIBERS. Science teachers in a school or technical college are admitted, on the proposal of two Members, to the use of the Library. They pay an annual subscription of one guinea.

Fuller particulars of the Royal Institution are given in the Prospectus, which can be obtained, with the forms of proposal for membership, from the Secretary, Royal Institution, 21 Albemarle Street, London, W.1. *Phone:* Hyde Park 0669 and 5716.

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